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THE PSYCHOLOGICAL REVIEW

ONTOGENY OF EMBRYONIC BEHAVIOR IN AVES: V. THE REFLEX CONCEPT IN THE LIGHT OF EMBRYONIC BEHAVIOR IN BIRDS

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INTRODUCTION

The concept prevailing in neuro-physiology and psychology, that instincts are innate combinations of reflexes while habits are postnatally acquired behavior patterns which are also integrated from simple reflexes, has been of late repudiated from several angles. In the first place, neuro-anatomy has failed to reveal that in the nervous system there are definite and more or less fixed anatomical patterns which are in definite correspondence with behavior patterns. Secondly, I have early pointed out (**13**, **14**) that the so-called instinctive behavior is too variable to be operated by any definite and stable structural arrangement of the nervous system. Indeed, so common is the fact of variability in behavior that even the most ardent instinct psychologist, McDougall (**19**), cannot help abandoning the reflex arc concept in favor of a non-mechanistic or vitalistic view of instinct. Thirdly, Lashley (summarized in **18**) and others have clearly demonstrated that the so-called 'learning process' does not involve the establishment of any definitely localizable reflex circuits in the central nervous systems corresponding to the 'learnt' act, as has been so commonly supposed by neurologists and psychologists. Fourthly, the reflex concept has been repeatedly

attacked by a certain school of psychology, namely, the so-called *gestalttheorie*, on the metaphysical ground of the supremacy of whole over parts. And, finally, and most important of all, is the fact revealed by recent studies of developmental behavior that the early behavior of most, if not all, vertebrate animals is characterized by massive movements, that is, movements which involve the body as a whole, rather than by separate or independent reflexes. In other words, separate reflexes of body parts are later differentiations from the early total and massive movements. Coghill's work on Amblystoma (**3**, **4**, **5**, **6**, **7**, **8**, **9**), has clearly demonstrated this point. The researches on the behavior of mammalian (**1**, **25**) and human fetuses (Minkowski and others, for bibliography see Coghill's (**6**) footnote) seem to point to the same fact. Thus, the traditional view that complex behavior is a result of the chaining or coordination of separate reflexes breaks down entirely in the face of these facts. Coghill further points out that simple reflexes never occur in the life of the individual; since local reflexes can acquire only a certain degree of independence, the apparently independent reflexes are still more or less under the influence of the organism as a whole. In the following pages we shall present facts concerning the behavior of avian embryos which in general agree with Coghill's observations. However, while Coghill in his studies of the behavior of Amblystoma and others in their studies of the movements of mammalian and human fetuses have not been able to determine the conditions under which the independent movements of body parts are originated, we have worked out such conditions in our observations of the behavior of the avian embryo.

The facts concerning the origin of such independent movements will also be reported and discussed in this paper. Furthermore, in the following sections we shall give reasons to show that while we are in agreement with Coghill with regard to the fact that independent movements of body parts come about as a result of differentiation or individuation from massive movements, in consideration of the development of behavior we attach much less significance to the process of

individuation of partial patterns from total patterns than Coghill has claimed, and we object to some of his neural interpretation of the dominence of the trunk movement over other parts of the body in Amblystoma, and, especially, his uncritical acceptance of the '*gestalttheorie.*'

Massive Movements in the Bird Embryo

The chronological order of the appearance of the movements of various parts of the body in the chick embryo has been reported in the first article of this series (**14**). It has been found that the earliest movements in the embryonic chick are the lifting and bending of the head. The ventral bending and the lateral twisting of the trunk come next. These are then followed by the movements of the appendages and of the tail. Oral movements, eyelid reflexes and movements of the eye ball appear last. Now, during the first half of incubation the movements of these various body parts are synchronized with the movements of the head and the trunk. This is especially true during the period of violent swinging movements and amnion contractions (**14**). During this period, with the exception of the head, which may occasionally make some slight movements without being accompanied by perceptible movements of other parts of the body, the body almost always moves as a whole. The limbs and tail move when the trunk moves. So do the eyes and the beak. It is extremely rare to observe the tail, the bill or the limb move independently. Even in the later stage of incubation in response to sound, touch, pressure and electricity, the whole organism is involved. However, independent movements of body parts, *i.e.* local reflexes, may also be observed in the latter half of incubation. The conditions under which local reflexes are originated will be described in a later section.

The Question of Neural Explanation of Massive Movements

In our study of the behavior of the bird embryo we have not attempted to find out whether there are any possible correlations between the growth of the nervous system and the

development of physiological reactions. Coghill, however, in his investigations of the behavior of Amblystoma has worked out such correlations in some detail. It may be well to review here his neural explanations of the massive movements in Amblystoma. Coghill reports that in the Amblystoma, the movements of the arms and gills are integrated with the trunk: The arms and the gills move only when the trunk moves. Based on his anatomical findings he explains the dominance of the trunk over the arm as a result of the fact that the motor nerve fibers which innervate the limb muscles are the branches of the same nerve fibers which innervate the muscle segments of the trunk. Such an explanation is highly plausible and one can hardly object to it on *a priori* grounds. But when we turn to Coghill's anatomical explanation of the dominance of the trunk over the gill, we feel that it is rather far-fetched. He says: "In a transverse section of the medulla oblongata of *Amblystoma* of the early swimming stage, the motor cells of the group which are to innervate the muscle of the gill are situated in the most dorsal part of the motor division of the nervous system. Large dendrites from these cells invade all sources of afferent impulses through the sensory system, the ascending trigeminal tract and the lateral line tract. They reach also into the zone of synapse between the commissural cells of the floor plate and the motor neurones that innervate the trunk muscles. The muscles of the external gills are therefore subjected to excitation not only by those afferent impulses which are on the way to the motor mechanism of the trunk, but also by those efferent impulses of the motor path which are on the way directly to the muscles of trunk. In general, therefore, by reason of these relations of the motor neurones that innervate the muscles of the gills, these muscles, after they have acquired sensitiveness to nervous excitation, would be expected to receive excitation from impulses that are on the way to the muscles of the trunk.

"Since the gills act with the trunk before they act independently in response to afferent impulses it must be that the motor neurones that control them establish functional synapses with the motor neurones that excite trunk movement

before they establish such relations with the afferent neurones." (**4**, pp. 31–32.)

I wonder if such an explanation as quoted above has any significant and specific meaning at all. It tells us nothing more than the fact that the nervous system is integrated. Of course, every one knows that every neurone is connected in one way or another with all and every other neurone of the nervous system and that impulses from certain neurones may reach some other neurones at the furthest distance from them. But the question is: is it necessary to explain the synchronism of movements of body parts in terms of specific neural connections? I took chick embryos from eggs and put them in warm p.s.s. Weak electric stimulation was applied to the tail. The tail moved simultaneously with eye-lid reflexes and the opening of the beak. Of course, it is possible to demonstrate anatomically that the nerves of the tail are in some way connected (through the central nervous system at least) with those neurones which control the eyelids and the oral muscles. But here again such a demonstration tells us nothing specifically. The point I am driving at is this: as long as the reactions of the embryo are found to be neurogenic (regardless whether the motor nerves are activated by sensory stimulation or by some endogenous agencies (**23**, **9**) and as long as the nerves controlling the reacting organs are integrated with the central nervous system, the synchronism of movements of different body parts is already explained. In other words, the mere fact that the nervous system as a whole is integrated is sufficient to account for the massive movements of the embryo; it is relatively unimportant to determine whether the synchronism of movements of two body parts is a result of direct synaptic connection, or a result of the fact that the two parts are innervated by the branches of the same nerve, or merely due to the fact that they are integrated through the central nervous system; for different body parts even without direct synaptic connections may move simultaneously. (The synchronous movement of the tail of the chick embryo, when stimulated by weak electric current, with the movements of the beak and eyelid is a case in point.)

I think that the most difficult and fundamental neurological problem is concerned not so much with anatomical explanations of synchronism of movements, since anatomically the nervous system is integrated from the start (to borrow Coghill's expression), as with the question why or how the so-called local reflexes are brought about in later stages of behavioral development in spite of the integrative nature of the nervous system from the beginning of behavior. This leads us to the question of the origin of local reflexes.

The Origin of Local Reflexes

While it seems now to be established from Coghill's studies on Amblystoma and from those of others on mammalian and human fetuses that local reflexes appear as a result of differentiation from the total behavior pattern, the exact conditions under which the local reflexes are originated thus far have not been reported. In the bird embryo we have found that there are at least two fundamental factors—intensity of sensory stimulation and environmental interference—which are responsible for the origin of independent movements of body parts.

a. Local Reflexes as Produced by Environmental Interference.—As has been stated, excepting the head, which may occasionally make some slight independent movements, local reflexes of other parts of the body are rarely observed in the chick embryo in the first half of incubation. During this period the embryo either moves violently, involving the whole body, or rests motionless. Even in the slight jerking of the trunk, other body parts are also involved. In violent movements of the trunk, not only the head, neck, tail and limbs, but also the eyelid reflexes and oral movements are involved. But after the 11th or 12th day of incubation, several local reflexes begin to gradually appear one after another. In the second paper of this series we have reported that by about the 12th day the yolk sac begins to come over the ventral side of the embryo, covering its ventral wall, the legs, the beak, the forehead and both eyes. In most cases a part of the right side of the body and of the right wing is also covered by the yolk

sac after about the 13th day (**15**). In this position, the dorsal side of the body and of the neck is in direct contact with the egg shell with the allantoise and egg membranes between them. It must be emphasized that even in the later stage of incubation the yolk sac which contains the yolk is still heavy; shortly before hatching, it weighs several grammes. These conditions greatly interfere with the movements of the embryo (**16**), the bending of the trunk and the lifting movement of the head are reduced to a minimum, the frequency and extent of the turning of the head and of the opening and closing of the beak are also diminished. During this period, jerking and wriggling movements are frequently observed. Such movements involve the body as a whole. But when the embryo is at rest, the clapping and opening and closing of the bill and movements of the eyeball and the eyelids may be seen to occur independently, that is, without the participation of the head or the trunk or the legs. Such independent movements of the bill and of the eye are observed more frequently when the beak and the right eye are not covered by the yolk sac. After the embryo has turned so as to lie lengthwise of the long axis of the egg (**15**), even wriggling movements are not observed so often. The bird, at this time, is seen only to make occasional jerking. But clappings of the bill and the eyelid reflexes and eye ball movements occur more often than before. Independent movements of the toes (scratching movements) and slight wing movements may also take place without the participation of the trunk. In fact, from about the 15th day to the time just before hatching, the embryo is motionless most of the time so far as wriggling of the trunk and movements of the head and legs are concerned; (wriggling is seen to occur, however, at the time of the protrusion of the neck downward and forward into the air cell). Eye movements become most frequent after that portion of the yolk sac covering the head and bill has been drawn into the body cavity.

If we consider the position of the chick embryo in the egg during the last few days of incubation, we could easily see why independent bill clapping, and eyelid movements (and to some extent, the scratching of the toes and movements of the

right wing) occur frequently, while movements of the head, the trunk, and even the legs are reduced to a minimum both in extent and in frequency. The body is large and heavy at this time, its back is in contact with the shell and its ventral wall as well as the forehead and bill are covered by a rather heavy yolk sac. The neck is doubly bent. After the yolk sac has been withdrawn from the head the membranes in the large end are directly applied on the right face. Besides, the liquid content in the egg is very much reduced. In this position the bird is virtually 'bound up' in the egg; free and extensive movements of the head, neck, trunk and extremities are greatly impeded. The movements of the bill and of the right wing are also checked, at first by the yolk sac and later by the membranes. Hence the movements of the bill are reduced to clapping and slight and infrequent forward and upward thrusting (**16**). The right wing, which is held tightly against the body wall, can only be drawn up and down. The only part of the body that can move without much external interference is the eye. These are the conditions under which the independent movements of the eyes, bill clapping and, to some extent, toes scratching and wing movements are originated. In other words, environmental interference with the movements of the head and, especially, of the trunk give rise to these independent local reflexes.

It must be emphasized here that the acquisition of the above mentioned independent movements is brought about by a gradual reduction of the head and trunk participation in the movements. The process of the reduction is so gradual that even at the end of incubation one can often observe an independent eye movement or bill clapping to be accompanied by some slight jerking of the trunk or of the head. In fact, the withdrawal of the head and especially the trunk participation in such independent movements is practically in every case incomplete even after hatching. Our observations agree with those of Coghill in Amblystoma in that local reflexes can acquire only a certain degree of independence; absolute independence does not occur in normal cases.

It must be pointed out also that in the bird embryo, even

after the local reflexes have acquired the ability to move independently, in violent movements the body as a whole is involved.

b. Local Reflexes as Determined by Intensity of Sensory Stimulation.—In the chick embryo in response to strong stimulation of sound, pressure or electricity, practically every part of the body is involved. But in many cases if the intensity of electric or pressure stimulation is reduced, local responses may be elicited,—that is to say that with very weak stimulation only the stimulated parts are involved in the response. In our observation of many embryos delivered in warm physiological salt solution, we found that in response to electric stimulation, the stronger the stimulation, the more parts of the body are involved, and that there seems to be a gradation of the strength of the responses of different body parts, that is, the further away from the stimulated area is the part which is involved in the response the weaker will be its response. It clearly indicates, then, that the degree of intensity of sensory stimulation is an important factor in determining whether the organism will respond with a total pattern or with a partial pattern, or even in determining how many and what parts of the body will be involved in a given response. In this connection it must be stated that although a visual stimulus in general can only elicit eye reflexes in the chick embryo, bright light which is suddenly introduced into the incubator may sometimes set the whole organism in motion. This is true both shortly before and after hatching.

The Origin of Alternate Movements of the Legs

Graham Brown (**2**) and Windle & Griffin (**25**) have observed alternate movements of the legs in fetal kittens. But as they were unable to make continuous observation on the same fetus in the normal uterine environment, the conditions which give rise to such alternate movements were not known to them. Consequently, Brown was led to the conclusion that progression in animals (walking, etc.) is a hereditary pattern, since it appears before birth. But the following observations on the origin of alternate leg movements in the bird

embryo will clearly demonstrate that the concept of hereditary behavior is generally employed as a cloak to cover our ignorance of the causes of origin of behavior:

As has been stated, in the chick embryo in the first half of incubation, movements generally involve the body as a whole; not only do the legs almost always move together with the trunk, but also both legs are flexed or extended synchronously. But after about the 11th day alternate movements of the legs begin to appear. This is brought about by the changes in the positional relation between the yolk sac and the embryo. We have reported elsewhere (**15**) that before the 11th day the embryo lies with its back on the yolk sac, but after that day the yolk sac, in normal cases, is pushed over to cover the ventral wall and the legs. As a result, the legs are folded down on the breast. Under these conditions extensor thrust of the legs is interfered with by the amnion and especially by the yolk sac which are pressing against the legs. Now in order to permit movements, one of the legs has to push up the amnion and the yolk sac, thus lessening the pressure of the yolk sac on the other leg and allowing the latter more room for movements. Thus, between the 12th and 15th days of incubation the embryonic chick is often observed to employ one leg in pushing up and supporting the yolk sac while the contralateral leg is making repeated flexions and extensions. Sometimes the right, sometimes the left leg is seen to be engaged in supporting the yolk sac. This is the exact condition under which the independent movement of one leg without the participation of the other is gradually acquired. And we conclude that this condition also gradually gives rise to the alternate movements of the two legs. For in those cases in which the yolk sac fails to cover the ventral wall of the embryo, alternate leg movements have never been observed. In other words, under the pressure of the yolk sac the alternating extension and flexion of the two legs is gradually acquired.

In the beginning, the reciprocating movements of the two hind limbs are not well timed. Truly rhythmic legs movements are observed generally by about the 17th day of incubation.

The Question of Neural Correlation with Independent Movements of Body-parts

As has been pointed out in a previous section, it is relatively simple to explain in neural terms the integrative nature of embryonic behavior, but it is far more difficult to explain neurologically why the so-called discrete reflexes do finally appear in spite of the fact that behavior is integrated from the start. At any rate, laboratory findings for such an explanation are not forthcoming. Minkowski (**20**) has suggested that the myelination might be responsible for the genesis of the independent movements of body parts. But Angulo y Gonzalez (**1**) has found that discrete reflexes in the fetal rat appear before myelination, showing that the theory which proposes to make myelinogeny a criterion of functional insulation of conducting path is not well grounded.

There can be no manner of doubt that there is some sort of change in the nervous system as a result of the appearance of the discrete reflexes. But precisely what such changes are remains to be worked out. Personally I will not venture to speculate on this question. But I will suggest just two points: (1) The neural explanation of discrete reflexes must be a physiological (central (?) or reciprocal inhibition or what not) rather than an anatomical one. Any attempt to find an anatomical basis of discrete reflexes is very likely to meet with failure. For anatomically, the whole nervous system is integrated from the beginning of behavior to the end of life. Furthermore it is a simple matter to explain the chaining of reflexes, as the concepts of the formation of new pathways and of the reduction of synaptic resistance through exercise will very well take care of the chaining process. But when we come to the question of the differentiation of partial pattern from the total pattern, the case is different. For, here, both of these two concepts break down entirely.

And (2) such changes in the neuro-physiology which are correlated with the independent part movements must be the result, rather than the cause, of the origin of such movements. For we have found that independent local movements appear definitely as a direct result of environmental interference

with total movements. Furthermore, as has been described, reduction in the intensity of stimulation can also produce independent movements of body parts. In this respect intensity of stimulation probably continues to play an important part throughout the life of the individual. For it is a common observation that even in adult life stimulation with high intensity generally throws the whole organism into activity, while weak stimulation effects only certain parts of the body.

Discussion

From our observations on the behavior of the bird embryo and from various other studies cited in the introductory section of this paper three points regarding the reflex concept seem to have been established:

(1) That there is no such a thing as a simple reflex.

(2) That behavior begins as a total pattern, independent reflexes or partial patterns come about as a result of later differentiation from the total pattern.

And (3) That behavior is too complex and too variable to be explained by the concept of chain reflexes.

Let us take up these three points separately.

(1) That the simple reflex concept is an abstraction was pointed out by Sherrington (**21**) many years before Coghill. It is true that in many of the textbooks on neurology, organ physiology and general psychology the fiction of simple reflex has been uncritically accepted and overemphasized. But for this Sherrington can not be held responsible. As a matter of fact during the past twenty years or so no leading student in neurology and behavior has ever taken the simple reflex concept seriously. Coghill's repeated assertion that he has failed to find in the Amblystoma any simple reflex does not seem to strike me as a new discovery. However, those who still hold fast to this fictitious concept of simple reflex must be reminded of the fact that even in such a simple movement as the Achilles reflex in the rat the whole nervous system is involved (**24**).

(2) Credit must, however, be given to Coghill for his first observation that in the Amblystoma behavior is integrated

from the start, while "partial patterns emerge within the total pattern." As far as I know, Coghill is the first person in point of time to announce and emphasize this fact. Judging from the studies in mammalian fetal movements and from our own observations on the behavior of the embryonic chick and other avian embryos, Coghill's principle seems applicable to all vertebrates. In the past we were wont to think that behavior appears first as separate reflexes and that only through later integrations are these reflexes linked together to form complex movements. But the facts of embryonic and fetal behavior in various vertebrate animals are now definitely at variance with such a belief.

Notwithstanding the fact that Coghill's observations in this respect are in agreement with mine, there are several points on which our views are widely different:

(*a*) As has been previously stated, I am inclined to think that to explain the total behavior pattern in terms of neural anatomy (that is, in terms of certain special connections between neurons) seems unnecessary or far-fetched, since all the neurons in the nervous system are interconnected in some way, and since body parts without direct neural connections may also move together, as in the case of the synchronous movement of the tail with the beak and eyes.

(*b*) Coghill has failed to study the environmental conditions which give rise to partial behavior patterns. This has led him to overemphasize the importance of the nervous system in the development of behavior at the expense of environmental and other physiological factors. On the other hand, while not denying the importance of the nervous system, we have demonstrated both in this and in the third reports of this series (**16**) that environmental conditions are a most essential factor in determining embryonic behavior in general and in the genesis of partial behavior patterns or discrete reflexes in particular. In this connection it is interesting to note that Coghill so far has not been able to report his morphological findings in the development of the nervous system in Amblystoma which he would regard as responsible for what he has called 'individuation of discrete reflexes.'

(*c*) I cannot agree with Coghill that his findings in any way tend to support the position of the gestalt view in psychology. The so-called *gestalttheorie* is a hybrid between the old fashioned mentalistic psychology and Bergsonian philosophy under the disguise of science. It attempts to build a psychological (philosophical) system around a unifying principle by the empty and somewhat mystical name of gestalt or configuration. This is an old trick of philosophers. In the sciences, on the other hand, no one would care to postulate a theory and then apply it to the whole universe. But this is precisely what the gestalters have been trying to do.

Whether behavior starts with movements of the organism as a whole or complex behavior is built up as a result of chaining or linkage of simple reflexes, is a question of fact. If the former is true, we have to analyse in detail the exact causal factors which are responsible for the genesis of the so-called discrete reflexes, as we have done in the case of the bird embryo. Such vague statements as 'whole dominates over parts,' 'partial patterns emerge within the total patterns,' 'qualities upon a ground' or 'configuration upon a ground' and the like are meaningless as well as useless. To be sure, every one would accept the notion of the 'organism as a whole.' But as men of science we ought to learn much more than this empty and somewhat idle statement. In other words, we have to know exactly what parts are correlated, and how. This can be studied only *by analytical and quantitative methods and by control experiments and can be described only in strictly physical and mechanistic terms.* But when one reads the gestalt literature, one cannot escape the impression that the analytical and quantitative methods of the exact sciences are superficial, that laboratory control is foolishness, and nothing but wholes.

(*d*) The problem of the development of behavior is not so simple as Coghill has thought. The question of 'individuation versus integration,' as Coghill expresses it, (**8**) is relatively unimportant in comparison with the question as to how the massive embryonic or fetal movements come to be reorganized in postnatal life into sequential order in accordance with environ-

mental conditions. The development of behavior is primarily a continuous process of reintegrating the bodily movements into serial responses. The individual acts, whether massive or discrete, are of no great significance unless they are hitched together into temporal order according to the demands of the environment. In other words, the development of behavior is fundamentally a continuous process of interaction between the organism and its environment, and as a result of such an interaction, the individual bodily acts are reorganized in various ways into serial order.

The point I am here stressing may be restated in this way: The individual is born with various sorts of embryonic movements which involve the organism as a whole. Now when this newly born organism comes into collision with a new environment, it is forced to rearrange these embryonic movements into various serial responses so as to meet the demands of the environment. In the rearrangement of such original, embryonic movements (that is, movements which originated in prenatal life) into temporal order both processes of 'individuation and integration' are involved. That is to say, in the process of reintegrating the massive embryonic movements into serial responses, certain of these movements have to be broken up (individuation) and then, are reorganized (integration) into new serial orders. The best illustration of this fact is to be found in the acquisition of speech. The new-born infant may utter certain vocal sounds together with total bodily movements. Now in acquiring speech the child has to analyse or individualize his original massive movements, selecting certain of the vocal acts, and then resynthesyze them into word or sentence responses.

From this point of view the question as to whether the earliest embryonic movements involve the body as a whole or are merely discrete reflexes is not of great concern to the student of developmental behavior. For such embryonic movements are merely the material or basis upon which and out of which various response systems in serial order in postnatal life are organized. Given the embryonic movements as basic materials, the fundamental problem which the

student of developmental behavior has to solve concerns the manner and causal factors of integration or reintegration out of such basic materials, into temporal behavior. Of course, he is interested in knowing whether the basic embryonic reactions are massive movements or discrete reflexes. But this question is trivial as compared with the question of the manner and causal factors of temporal integration.

The question of 'individuation versus integration' in the development of behavior has been unduly emphasized by Coghill and by Herrick (**10**). Both of these authors appear to have entirely neglected the temporal aspect of integration, which is by far more fundamental than the question concerning the nature of the basic embryonic movements which are the building material of temporal integration. Neurologically it may be important to decide whether complex behavior is built up of simple reflexes, or discrete reflexes are differentiated from a total pattern. But behaviorally, our problem of development is much more fundamental and complicated than this. And, to repeat, the process of temporal organization of movements, which is the most fundamental aspect of behavior development, involves 'integration' as well as individuation.

(3) Some ten years ago (**12**, **13**) I pointed out that behavior is so variable and so complex as not to permit any explanation in terms of definite or fixed anatomical arrangement of the nervous system or in terms of chain reflexes. Recently Tolman (**22**), Herrick (**10**) and Lashley (**18**) have also emphasized this fact. In this connection it may be well to bear in mind that while it may be possible to discover definite anatomical correlates of the nervous system with embryonic or fetal movements which are relatively simple and more or less uniform, any attempt to find such correlates in postnatal behavior is bound to fail in view of its complexity and variability.

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'HYPOTHESES' IN RATS[1]

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It has long been observed that the usual curve obtained in the establishment of a sensory discrimination habit differs characteristically from the 'typical' maze learning curve. Where the typical maze curve shows an initial period of rapid progress, then a period of negative acceleration to mastery, the curve representing discrimination learning shows an initial period of practically no progress, then a period of positive acceleration to complete mastery. The significance of this latter curve has never been made clear.

Another interesting fact in connection with discrimination experiments that has also been observed again and again is that the animals, before mastering the problem, often tend to build up various 'position habits.' Habits such as going to the same side of the discrimination box at each trial or alternating from side to side with each successive trial have been observed by various experimenters.

The relation of the latter fact—the setting up of position habits—to the former—the peculiar shape of the learning curve—apparently escaped notice for quite a while, yet an investigation of that very relationship will yield results which we believe necessitate a reconsideration of our descriptions of the learning process, and the reëxamination of some of our 'granted' postulations.

It is rapidly being recognized by many writers on the theory of learning, that our older concepts are inadequate and have outlived their usefulness as even fruitful working hypotheses. Very few serious experimenters would now wish to defend the concept that a learned response consists of a

[1] We wish to express our gratitude to Professor Edward C. Tolman and Assistant Professor Robert C. Tryon for their interest, encouragement and criticism in the preparation of this paper.

number of independent S–R connections. Gradually, the concept that a learned response is an integrated *unity*, a systematic whole, has gained the acceptance of most animal psychologists. But this change in view is one that has been applied only to the completed act, to the behavior of the animal after he has 'learned' a given series of responses. The status of the behavior of the animal before the act has become 'perfected,' for the most part, has remained unchanged. In other words, while we have come to redescribe the learned act we have not yet changed our description of the learning of that act.

Almost every one of the older theories of learning has made some room for the factor of 'chance' in its description of the learning process. Most outstanding in this respect is the older orthodox behavioristic theory. Every one is familiar with Watson's proof that chance alone could insure the animal's entry into the correct alleys of any maze more frequently than into the blind, with the inevitable result (the efficacy of frequency being assumed) that the animal would soon learn to enter the true paths only. Watson, of course, is not the only one who leaned heavily upon the concept of 'chance.' Almost every description of animal learning includes such phrases as '*random* exploratory movements'; 'chance entrances'; 'chance errors.' It is either implicitly assumed or explicitly stated in most descriptions of learning that in the beginning of the process the animal is a 'chance' animal. His responses are without purpose, without form, and without meaning. Helter-skelter trial and error seems to be the rule at first, and then, after such behavior has eventually led the animal to experience the 'correct' pattern, the various laws of learning step in to stamp in the correct responses and stamp out the incorrect. At this point the various theories of learning might differ somewhat, but as to the first part of the process they all agree.

Perhaps the most outstanding and militant attempt at a theory of learning without any 'chance' suppositions in it at all is the Gestalt theory. In a convenient summary of the Gestalt theory of learning, Koffka writes, "To cut a long story short, we find at the beginning, in our most elementary

reactions . . . in training and in intelligent performances, unitary, articulate, meaningful wholes . . . . Development starts . . . with structures. Development proceeds by transformation of such structures. Gradually, by a number of . . . leaps and bounds we achieve different orders, different articulations, different meanings." [2]

The difference between such a description and the other formulations is at once obvious. In this latter theory there is no room for 'chance.' It is implied that the animal, placed in a new situation and confronted with new obstacles which are to be overcome, does not rush about on mere chance urgings, but his very first response is a meaningful, intelligent one; meaningful in that it betrays some plan, some *unifying* aspect. With learning, the animal substitutes one unified series of responses for another until he eventually adopts the 'correct' one.

It is our opinion that the Gestaltists have perhaps gone beyond their facts when they attempt to account for the genesis of these various responses on the basis of some sort of inevitable and predetermined bond between stimulus field and response,[3] but their formulation, considered as a description of learning is one worthy of consideration. Essentially, it does to each step in the learning process what has already been done to the end result of the process, *every* series of responses is treated as an integrated, unitary whole.

As has been suggested above, a study of the results obtained from discrimination experiments is of extreme significance in considering this very problem. A brief analysis of the results of the usual discrimination experiment will show this quite clearly.

When the number of errors is plotted against the number of trials, in drawing the usual discrimination-learning curves, the curve representing the number of errors made remains on

[2] K. Koffka, Mental development, Psychologies of 1925, Clark University Press, Worcester, Mass., p. 136.

[3] The Gestaltists, it seems to us, lay themselves open to the same criticism that they make of the 'connectionists.' If one reads 'stimulus-configuration' for 'stimulus' and 'gestalt of responses' for 'response' one can formulate the gestalt hypothesis of S-R relationship in the very same way as the strictest behaviorist.

the 50 percent line for approximately the first three-quarters of the trials, then it very rapidly descends to the no-error level. When the curve is at the 50 percent line it is at once assumed that the animal is making '*chance*' responses. That is, of course, good reasoning, for if the animal *were* making purely unsystematic responses that is just what we would expect, *i.e.*, he would enter the wrong alley as often as the correct one. After a number of chance responses had been made the various laws of learning could then step in (having gotten a foothold by the aid of chance) and the error curve would then drop to the zero line. So far, it is seen, the learning curve supports the older proponents of 'chance' as an important factor to consider in describing the learning process. However, when we look for a possible relationship between the shape of the curve and the tendency of the animal to form various interfering position habits we can at once see the possibility of a different interpretation of the data.

Lashley, in his "Brain Mechanisms and Intelligence" was perhaps the first one to suggest a possible relationship. "There are many indications," he writes, "that . . . in the discrimination box, responses to position, to alternation, or to cues from the experimenter's movements usually precede the reaction to light and *represent attempted solutions* that are within the rat's customary range of activity . . . . The form of the learning curve is the more significant when considered in relation to such behavior . . . . It suggests that the actual association is formed very quickly and that both the practice preceding and the errors following are irrelevant to the actual formation of the association." This suggestion at once puts a different light on the whole matter. Perhaps the animal, during the *first* part of the process is not running in an haphazard, chance manner, but is performing a systematic, unified, series of responses! But Lashley is quick to point out that "there is no present way to record such behavior objectively, and I can present the description only as an impression from the training of several hundred animals in these problems." [4]

[4] K. S. Lashley, Brain mechanisms and intelligence, University of Chicago Press, Chicago, Ill., 1929.

Supposing, however, that Lashley's suggestion, which carries with it the essential concepts of Koffka's formulation of the learning process, were true, what kind of a learning curve should we expect? Obviously enough, the very same sort of a learning curve should result as would be predicted if the animal were running by 'chance' alone! A brief consideration of the technique employed in the discrimination experiment will show why this should be expected. The experimenter, to make certain that the animal is discriminating on the basis of the experimentally controlled stimuli when making the 'correct' choice and not on the basis of the spatial position of the alleys, so arranges the situation as to make it impossible for the animal to make more than an average of 50 percent correct responses if he persists in going to the same side of the box every trial. It follows then that no matter how systematic the animal's choices may be (on the basis of position) his *error* curve would perforce be on the pure chance line. It is at once obvious that we cannot argue from the locus of the error curve as to the lack or presence of system in the animal's responses. Such a curve tells us very little about the animal's behavior. More detailed analysis must be made before we can know anything at all about what the animal is doing.

The present paper presents a discussion of some data, culled from various experiments involving the setting up of discrimination habits in the white rat, which were examined in the light of the foregoing discussion. The method of analysis adopted in treating the data is simple and obvious. The method, however, has led to results which, we believe, are crucial for the matter under consideration. It is not the purpose of this paper to present the data in any detail; [5] we shall merely indicate the nature of the evidence and discuss its applicability to the problem at hand.

Nature of Experimental Evidence

The data given here and the figures presented have been taken from three different experiments carried out by the

[5] Detailed reports of these experiments are now being prepared and will be ready for publication shortly.

writer during the past two years. The first experiment was carried out in the psychology laboratory of Washington Square College, New York University, during 1930–31; the second and third were performed in the laboratory of the University of California.

In all three experiments the discrimination technique was used. In the first two experiments the animal was required to set up specific sensory discrimination habits, and in the third, the animal, while presented with a similar experimental situation, was given no opportunity to solve the problem; that is, there was no consistent relationship between any sensory stimulus and the correct alley. The apparatus used was the same for all three experiments. In general it consisted of a multiple-unit discrimination box, with four successive compartments similar to the box described by Stone [6] but with several important modifications (which, however, need not be considered for our present purposes).

Experiment I

Forty animals were run in this experiment on two different discrimination problems. The first problem was a brightness discrimination and the second, a hurdle discrimination. In the hurdle discrimination the animal was presented with two equally lighted alleys at the entrance to one of which was inserted a rod which the animal had to climb over in order to enter. The alley containing this hurdle was the 'correct' alley. This discrimination proved to be much more difficult than the brightness discrimination, and because it thus gives long learning curves we shall here discuss the results of this hurdle discrimination only.

Inasmuch as the method of treating the data in this experiment is essentially the same as that used in the other experiments it would perhaps be appropriate to describe it in some detail at this point. Throughout the analysis of the data we considered individual records only. We stress this fact because it is of fundamental importance as will be apparent

[6] C. P. Stone, A multiple discrimination box and its use in studying the learning ability of rats, *J. Genet. Psychol.*, 1928, **35**, 557–573.

later on. An analysis of the data such as we herein describe would be impossible if we were to throw together the records of the entire group of rats, strike hypothetical group averages and plot hypothetical learning curves.

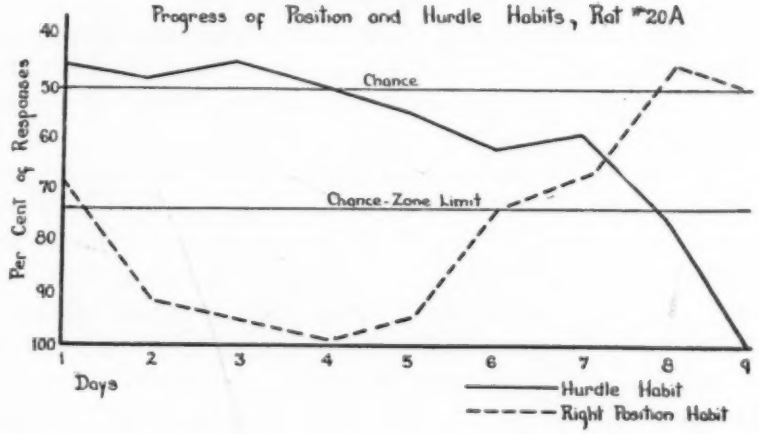
For each rat then, the following items were determined: (1) the number of 'errors' the animal made during each series of 40 discriminations (one day's work *i.e.*, 10 runs, four successive discriminations each); (2) the number of turns to the right the animal made each day; (3) the number of turns to the left; (4) the number of turns in keeping with an 'alternating' scheme and (5) the number of responses in conformity to a 'perseverative' scheme. From such data a series of 'learning' curves were plotted for each rat. First the usual 'error' curve was plotted, then, on the same ordinates, a curve successively representing the number of turns to the right that the animal had made, was drawn. The same was done with the other possibilities, *i.e.*, the animal's 'alternating' and 'perseverative' behavior. Such a figure should give us a somewhat detailed picture of the animal's performance on the successive days of the learning process. The following discussion will perhaps be clearer if we refer to a concrete case. In Fig. 1 is presented one of the resulting graphs. The horizontal line running across the graph at the 50 percent point indicates the locus of pure 'chance' responses. By the formula

$$SD = \sqrt{\frac{PQ}{N}}$$

(this formula is used since the results are here presented in terms of percentages rather than in terms of number of items) we can determine the limits beyond which the percentage of any set of responses would hardly be expected to go if chance alone were operating in forcing those responses (*i.e.*, 50 percent $\pm$ $3SD$). Doing this we find the upper limit to be on the 73 percent line. If, therefore, any curve should go beyond this limit we can be reasonably certain that such a change was not a chance fluctuation but due to some *systematic* cause. This limit is indicated in the figure by an horizontal line. The solid curve represents the percentage of errors made

on successive days. This curve is the typically obtained discrimination curve. For the first seven days the curve remains well within the pure 'chance' zone, then it very rapidly drops to a point indicating complete mastery. The broken-line curve represents the percentage of turns the animal made to the right side of the box. It is the consideration of this curve which is most important. This curve vitally changes the usual description of the learning process. This curve does not remain within the 'chance' zone at all, as it should have done were the animal, during the first seven days, responding in an unsystematic fashion. During this so-called

Figure 1 (Exp. 1)



'chance' period, the animal adopted, brought to perfection, and 'tried out' a *systematic right-position-habit.* After a period of running in this *unified* manner, the animal gave up his right-position-habit, and adopted the hurdle habit. This curve shows definitely and unmistakenly that the animal is *from the very first* behaving in a *unified* systematic manner. Throughout the whole learning process we find very little room for haphazard behavior. Unified behavior, rather than hit and miss activity seems to be the rule.

As will be shown in the detailed report of this experiment, to be published later, such a curve is by no means an isolated one, such behavior seems to be universal for the learning

process. We might state that the results of the experiment show that actually the total number of animals displayed 36 definite attempts at 'wrong solutions' (*i.e.*, systematic spatial responses).

Experiment II

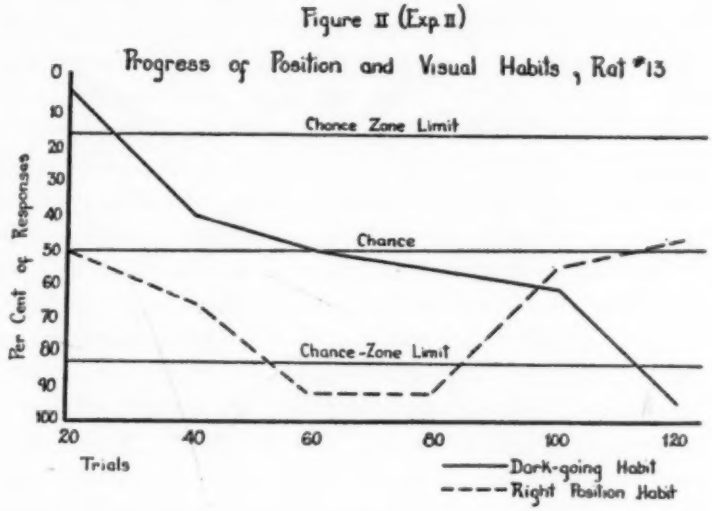
The evidence we cite here is incidental evidence which was obtained in the course of analyzing the data for other purposes. The experiment was primarily designed to repeat that of Fritz.[7] As such it involved the training of a number of animals (10) in a series of antagonistic visual habits. The animals were first trained to respond positively to the brighter of two stimuli and negatively to the darker. After they had met the norm set for learning (19 correct discriminations out of 20 consecutive choices) the animals were immediately set to learn the opposite response, *i.e.* now the darker alley was correct and the brighter alley, the incorrect one. This was continued for 22 reversals.

Such an experimental situation gives us a good opportunity to further test out the validity of our description of the learning process. When the problem is reversed for the animal and he now must learn to reverse his responses we get a sort of double-headed discrimination curve. At both the beginning and the end of the learning process the error curve is far removed from the 50 percent line, but the central portion of the curve remains on the 50 percent line for quite a period. In the first part of the performance the error curve is at the hundred percent line, since the animal is still responding on the basis of the first set-up which has now been reversed on him; in the last part, the error curve is, of course, at about the zero percent line since the animal has now mastered the required problem, but in the central part the animal appears to be responding in no systematic way at all. If the animal, however, does not behave *at any time* in an unsystematic manner we should be able to find here at least three systematic series of responses. First, the animal responds consistently to the 'incorrect' visual stimulus; after that, during the so-

[7] F. Fritz, Long-time training of white rats on antagonistic visual habits, *J. Comp. Psychol.*, 1930, 11, No. 2, 171–184.

called period of 'chance' responses, the animal makes a systematic and unified spatial response; finally the animal attempts a third systematic response, this time to the 'correct' visual stimulus.

Figure 2 graphically presents the performance of a single animal on his third reversal. The scheme of presenting the data is the same as that used in the previous figure (in this case, however, each point on the abscissa represents 20 choices instead of 40). Even a superficial inspection of the figure



shows how clearly the actual data are in agreement with the thesis developed above. What we have here is a *series of systematic attempts at solution.* Instead of describing the learning process as going through the following periods: (1) systematic but unsuccessful behavior, which, through the operation of the various 'laws of forgetting' becomes (2) haphazard and random, and which, in turn, again through the effects of the various 'laws of learning,' becomes (3) unified and successfully, we would rather have to say: (1) one systematic response (to the Light) is given up for (2) another systematic response (to the Right), which is given up in turn for (3) yet a third systematic response (to the Dark) and

so on until some 'most satisfactory' system of responses is achieved.[8]

Experiment III

This third experiment was specifically designed as a further control. It might be argued that the various spatial systematic patterns of behavior were not definite 'attempts' of the animal at solution, but were mere blind habits which the situation, because it might not have been perfectly controlled, forced upon the animal. That is, the right side of the box, or the left side, etc., might have had, in each unit, some local, specific advantage to which the animal was responding; or criticism might be made of the order of presentation of stimuli. The right side of the box may have been left open at more favorable moments than the left, or vice versa. Further, such phraseology as 'attempts' at solution implies that these responses are in some degree initiated by the animal himself and are not entirely due to the presented situation. Such implications need further experimental justification. To meet all these criticisms the following experiment was carried out: the animal was presented with the discrimination box situation but the stimuli were so controlled, by irregularly changing the 'correct' alley from light to dark and left to right, as to make it impossible for him to 'solve' the problem. If the animal persisted in taking the lighted alleys he would make but an average of 50 percent correct responses, the same would result from taking every right side, left side, alternating or any other systematic response. Now, in this situation, if the animal depended entirely upon the immediate external situation for

[8] It is not within the province of this paper to inquire into the factors which 'cause' one response to be given up for another nor which 'cause' one response to persist. Nothing finalistic is meant by the term 'most satisfactory,' nor is it used in any explanatory manner. Perhaps the least objectionable interpretation of a 'most satisfactory' response is that response which requires the least expenditure of effort to reach a necessary goal. Such a concept as has been developed by Gengerelli is here meant.

What we are most interested in in this paper is obtaining merely a description of the learning process on the psychological or *behavioral* level—on the purely descriptive level; and we are not postulating any fundamental physiological or philosophical principles. That the psychologist may very well content himself with treating behavior *qua* behavior and thus fulfill his function is the point taken by many of our present writers. See E. C. Tolman, 'Purposive Behavior,' Chap. I.

the setting-up of any unified way of behaving, he should show absolutely no unitary, consistent behavior. Further, if he *should* adopt some apparently consistent reaction (as a result of some chance, uncontrolled differential aspect of the stimulus field) he should certainly show no change from that behavior to another unitary behavior. On the other hand, if even here we get the same sort of systematic changes we have some more evidence that these various 'false solutions,' 'attempts' and so on are real, valid, systematic efforts on the part of the animal to solve his problem.

The results from this experiment are highly encouraging.

Figure III (Exp. III)

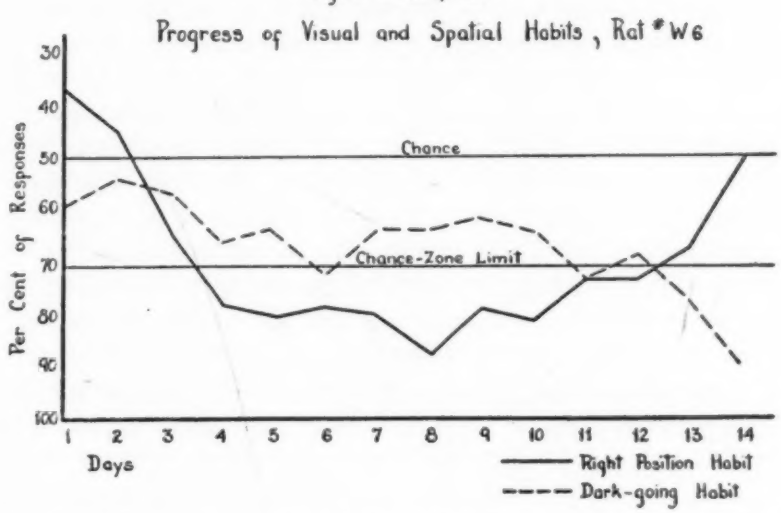


Fig. 3 is a sample graph of the kind that we have obtained. The same story is told here that has been told by the other graphs. We first see the animal adopting a Right-going response, that, after a period of days is given up and another consistent response is attempted—a Dark-going response. The figure is clear and its implications need no further discussion.

Theoretical Implications and Summary

It has been impossible to refrain from pointing out the theoretical significance of such results throughout the discus-

sion of the data. All along we have stressed the possible value of these results in helping us *extend our notions of the nature of the learned act to the entire learning process.* The animal, in adjusting to a change in his environment (learning) goes about it in an orderly, systematic manner. To the animal any new situation is not a confused, meaningless conglomeration of sensory impressions to which he makes confused, meaningless uncoördinated and unrelated responses. The animal is not altogether a victim of his immediate environment in the sense that each specific reaction is the result of a specific, momentarily-acting stimulus. He brings to each new situation a whole history of experiences. These experiences the animal is ready to apply. *From the very beginning*, perhaps, the animal goes about solving his problem in a straightforward, comprehensive manner wherein each response is not to be considered as a *Ding an sich* but as a meaningful part of his total behavior. The animal, in executing a series of movements which we call 'perfect,' 'errorless,' 'learned,' 'integrated,' is not doing something which has arisen from a series of 'imperfect,' 'unintegrated,' 'chance' responses. He is now merely running through a different set of integrated responses, which series of integrated responses were *preceded by other just as integrated responses.*

Such responses, 'false solutions,' 'early systematic attempts,' etc., we have dubbed with the dubious name of 'hypotheses.' The use of the term 'hypothesis' to describe such behavior is a confession of failure. We have been unsuccessful in finding any one term which might adequately describe such behavior yet not carry with it the stigma of being fantastically anthropomorphic. The word 'hypothesis' at first blush seems to violate every canon of the 'scientific' objective animal experimenter, and in a sense, of course, it does. It is extremely unfortunate that we find it so difficult to get away from the historical meaning of words. 'Hypothesis' carries with it all the mentalistic implications that have become associated with such words as 'reasoning,' 'consciousness' and many others. 'Hypothesis' as we wish to use it in this instance, however, need not and does not do so. Just as

it is perfectly legitimate to define 'consciousness' in purely behavioral and objective terms, so should it be with any other word which might conveniently serve any scientific purpose.

In a sense though it is the objector to the word 'hypothesis' and other words of that sort, when applied to animal behavior who needs to apologize for being non-scientific and non-objective. The individual who admits the use of the word 'hypothesis' when describing human behavior and shies at its application to animal behavior is really postulating a fundamental duality between the two kinds of behavior. We have not found it necessary to do so, and any concept which we may apply in describing human behavior we feel free to apply to animal behavior. We can do so only if we define all our concepts in *purely objectively observable behavioral terms.* When the human individual *behaves* in such and such a way we say he has an 'hypothesis'; when a rat *behaves* in the very same such and such way we must also say that he has an 'hypothesis.' However, we are primarily interested not in defending our terminology but in describing certain behavior. The term 'hypothesis' has merely been chosen as a convenient tag for such behavior, convenient because it carries with it the following behavior characteristics: (1) behavior which is systematic; (2) behavior which is purposive (displaying an 'if-then' character); (3) behavior which involves some degree of abstraction; and finally (4) behavior which does not depend entirely upon the immediate environment for its initiation and performance.

(1) That the first characteristic must be present in human behavior before we grant that the individual is working on an 'hypothesis' needs no argument. Chance and unrelated acts are not characteristic *behavior* acts of an 'hypothesis.' We have repeatedly seen that in the animal learning process we also find systematic behavior. Further discussion should not be necessary.

(2) When we say that an individual is working on an 'hypothesis' we at once imply that he is behaving in a purposive, 'if-then' manner. An 'hypothesis' must be put to experimental test, an 'hypothesis' is something that must be

verified before it is persisted in. If the hypothesis does not lead to certain expected results it is soon dropped. '*If* this attempt is correct, *then* I should get such and such results, if I do not get such and such results then I must change my behavior.' The 'then,' the anticipated consequences, determines the desirability or goodness of the 'if,' the behavior. The rat, in the maze or in the discrimination box, behaves in the very same way. The most recent, significant and exciting work in animal experimentation, we believe, has conclusively shown that the end result, the goal, is an extremely important causal factor in determining the animal's behavior even at points far removed from the goal spot. The experimental evidence and the theoretical implications of such work is too well known to necessitate repetition. The work of Tolman and his students are brilliant examples.[9] Remove or change the goal and you remove or appropriately change the behavior. The animal's behavior does meet this requirement of the term 'hypothesis.' A re-examination of Fig. 2 will demonstrate that fact. The animal systematically enters every lighted alley, which attempt, he discovers, does not lead to the desired effects. That the animal's behavior, the 'if,' was conditional and dependent for its continuance upon verification by the goal is demonstrated by the fact that he *changes* his behavior and tries another response and another until he finally gets one which *is* verified by the efficient attainment of the goal-object.

(3) The third attribute of the term 'hypothesis'—the 'abstractive' nature of such behavior—is perhaps another way of saying that the animal gives a unified performance; yet a bit more is meant than that. Only when we have abstracted, from all our data, some general method of consideration of such data can we be said to be working upon an hypothesis. A scientific hypothesis attempts to formulate a general and universal law to give meaning to a large number of special cases.

[9] Throughout this paper we have been guilty of using concepts that are 'verboten' by the objectivist. That one can and *must* do so and yet not sin against a scientific conception of psychological data is adequately demonstrated in Professor E. C. Tolman's recent publication 'Purposive Behavior' to which work we are indebted for many of the concepts here advanced.

The animal's behavior as described above shows that very thing. This can be better illustrated from the results of maze experiments. Tolman and Honzik, Dashiell and others have clearly shown that the animal, in threading his way through a maze, reacts to each individual choice point in a common way. The animal very rapidly learns that no matter what turns he has to take, food is in the *general* direction of the left, and as a result he will enter more blinds which point in that *general* direction than any other blinds. Tryon, in an extensive series of researches has more specifically attacked this very problem and his results leave little room for doubt as to the 'abstractive' nature of the animal's learned act. His results indicate that the specific and local stimuli at the various choice points have very little significance in determining the animal's behavior at these points. The animal can do without them; the animal is responding in a general way to some general characteristic of the total situation.

If we refer again to any of the figures presented here we see the very same thing. At first, the animal pays attention only to the spatial characteristic of the alleys (Figure III) no matter which alleys he is confronted with, he then disregards this and pays attention to (reacts to) the element of brightness, again no matter where the alley may be.

(4) Where does the animal 'get' these 'hypotheses'? The Gestalt theory has one answer. Such behavior, or any behavior is an inevitable result of some stimulus configuration. The situation *forces* a specific response. The relationship between stimulus-field and response is as definite as it is mysterious. We do not wish here critically to discuss such an interpretation, we have as yet not enough evidence on the problem of the genesis of behavior units, but the word 'hypothesis,' as commonly used, carries with it some implication of its origin and it is only that implication with which we shall concern ourselves. When we say that an individual has an 'hypothesis' we imply that the individual is contributing something to the situation. His behavior is not something forced upon him by the immediately presented stimuli. He has taken the problem-field and has brought to bear upon it all his past ex-

periences. His 'hypothesis' originates in some degree *from within himself*. The situation is *his* situation because it consists of both the external data and the individual's personal data. An 'hypothesis' is the individual's interpretation of the data, it is not a phenomenon deriving from the presented data alone.

The orthodox description of learning treats the animal's behavior as an event which is entirely forced *ab extra*. According to that concept the animal is a plastic individual whom the given specific environment molds as it will. The results of experiment III show how untenable is such a view. It will be remembered that in that experiment the environmental influences which might presumably force specific patterns of behavior were so controlled as to be ineffective. Neither the 'laws' of frequency nor of effect could possibly play any role in setting up any definite form of response, and yet the animal did show such behavior. These 'hypotheses' then must be *initiated* in part *by the animal himself*. They are the animal's interpretations of the data.

Summarizing, we may point out that in the face of all recent experimental evidence, helter-skelter trial and error learning must go by the board as a valid description of the learning process. We must describe the early, 'unlearned' behavior in the same manner as we have described the 'learned' act. Learning consists of changing from one systematic, generalized, purposive way of behaving to another and another until the problem is solved. The learning process *at every point* consists of a series of integrated, purposive behavior patterns.

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WEBER'S LAW AND THE FECHNERIAN MUDDLE [1]

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Doubtless it is rank heresy to raise the question whether the publication of Fechner's *Elemente der Psychophysik* in 1859 and 1860 has been a net gain or a net loss to the cause of science. Possibly without it E. H. Weber and Weber's law would have remained unknown to the present day. The work also gave prominence to the fact of the difference-threshold, and set forth experimental procedure and quantitative methods for threshold measurement and computation which have proved to be the basis of much present-day practice. Unfortunately these things, and other things of value in the book, are included in little less than 900 pages of German, none too easy as such, which also include an enormous amount of speculation, and which, in general, go into the elementary exposition of mathematics and physics more than would seem to be necessary. Concerning this last, we may admit that it might have been necessary at the time the book was written. In view of the prominence which Fechner achieved as a pioneer in this work, it is also unfortunate that the entire book has never been translated for the benefit of the English-speaking scientific world, for the effect that this work has had on the thinking processes of numerous members of the scientific fraternity is nothing less than deplorable. I base this judgment on expressions which appear to have been derived by hearsay, originally from Fechner's *Massformel*, which expressions have come to me in one way or another in the course of twenty years' theoretical and experimental study of the subject. Helmholtz's acceptance of the *Massformel* in modified form [2] has done nothing to stem the tide of misapprehension.

[1] From the Laboratory of Applied Bio-Physics, Oscar Johnson Institute, St. Louis, Missouri.

[2] H. v. Helmholtz, Physiological optics, English Translation, Vol. 2, p. 175 ff.

Weber's law is a mathematically definite statement. In effect, it deals with the relative values of any two stimuli, of magnitudes I_1 and I_2, whose difference is just perceptible to the sense-organ to which they are presented.

Before going further, something is to be remarked upon the meaning of a term implied in the above, that is, the term: 'least perceptible difference.' At its face value, this expression can only mean that the difference in question is perceptible, and that any smaller difference is not perceptible. The term thus necessarily means a deduction, implicit or overt, from not less than two observational data. Possibly either of these might be designated 'an immediate fact of consciousness,' or something equivalent to this; but the deduction is not immediate.

By whatever form of experimental technique the threshold is determined, it would appear, first, that the criterion of 'perception' is overt behavior, of some sort, on the part of the subject; and second, that the final threshold value calculated from the immediate data would represent a stimulus or a stimulus-difference which would prove to have been sometimes perceived and sometimes not, by the criterion used. Indeed, the stimulus represented by the exact final value need not have figured in the experimentation at all.

It is hard to understand how, since the time Fechner outlined the statistical method of arriving at a threshold, anyone could continue to use such terms as: least perceptible difference, greatest imperceptible difference; or their equivalents: just noticeable and just not noticeable difference. No such thing appears from the statistical data, and it may be suspected that these expressions arose in connection with threshold determinations involving the method of adjustment; in which it is admittedly obscure in just what way the subject arrives at a setting of the apparatus, and in which it is more than probable that the deduction, mentioned above, has already been made by the subject before he announces the adjustment made. In the last analysis, a threshold is a stimulus-magnitude selected *by inference*, from assumptions as well as facts, as the one associated with a standard probability of

characteristic response. The value reached will differ according to the experimental setting and technique by which the data are obtained, the method of computation used, and the standard which is adopted as the criterion. All of these are implicated.

Weber's law has been stated as follows: The least amount that must be added to any stimulus, in order that the difference may just be sensed, is a constant fraction of that stimulus, whatever its magnitude may be. *Of course* this proposition is not true at the absolute threshold, where the original (smaller) stimulus is zero. And the converse of this may easily be shown: *if this relation holds without limits, the absolute threshold must be zero.*

The most concise statement of Weber's law is:

$$\frac{I_1}{I_2} = C, \tag{1}$$

in which I_1 and I_2 are the greater and the less of the two stimuli respectively, and C is a constant, necessarily finite. This statement covers the verbal statement above, as well as every other unequivocal statement of Weber's law that has been made. For illustration, by subtracting 1 from each member of the equation above we have

$$\frac{I_1 - I_2}{I_2} = C - 1, \tag{2}$$

which becomes

$$\frac{\Delta I}{I_2} = c \tag{3}$$

if we designate the stimulus-difference by ΔI and use a new constant c, equal to $C - 1$. Equation (3) then is a mathematical statement of the relation stated verbally above.

Now it follows directly from (1) that whatever finite value may be assigned to I_1, a smaller stimulus I_2 may always be found which will be just noticeably different from I_1, and which will also be finite: I_2 can be zero only when I_1 is also zero. In short, it follows from the extension of Weber's law to the zero of stimulation that there is no finite absolute thresh-

old. The importance of this deduction lies in the fact that Fechner appears to have overlooked it, and thereby to have led himself to certain weird conclusions regarding negative or 'unconscious' sensations (*unbewusste Empfindungen*) merely by giving the false name 'absolute threshold' to an arbitrary finite constant which appears in his *Massformel* as the result of integration. Since the *Massformel* was derived from Weber's law, unlimited, the constant can have no such meaning, and its mathematical effect is merely to fix an arbitrary zero, as a reference-point, in an infinite scale of putative sensation-values.

The relationship stated in Weber's law holds true in some cases and within limits. While it is not the primary intention to discuss these limitations here, it is to be remarked in passing that *the 'law' is far from being universal, and that the exceptions to it are neither negligibly small, nor are they unimportant. Moreover, in the case of vision at least, these exceptions are not limited to the extremes of high and low intensity—when the sense-organ is just beginning feebly to respond to its adequate stimulus, or when it is beginning to be overloaded and pressed to the limit of its capacity.* A specific relevant case was experimentally examined and reported upon by the writer some years ago.[3] The result showed a complete breakdown of Weber's law, owing to nothing but the simple fact that it fails to take all of the relevant variables into account.

In much of the experimental work that has been done with visual thresholds, the two compared stimuli have together occupied an angular area of only a few degrees in the visual field. This extent is exceedingly small when compared with the entire visual field, which is perhaps two thirds of a hemisphere, centered at the eye. In much of the work done, including the notable work of Koenig,[4] the compared stimuli

[3] P. W. Cobb, The effect on foveal vision of bright surroundings—IV, *J. Exper. Psychol.*, 1916, 1, 540–566. It is also interesting to note in this connection that Fechner's own lifted-weight experiments fail to prove Weber's law. He is compelled to explain these deviations away in a manner which leaves the question very much as he found it.

[4] A. Koenig, Experimentelle Untersuchungen über die psycho-physische Fundamentalformel in Bezug auf den Gesichtsinn, *Gesammelte Abhandlungen zur physiologischen Optik*, Leipzig, 1903, pp. 116–134 and 135–137. Data reprinted in Helmholtz, II. Aufl., pp. 405–408.

were two parts of such a small bright field, seen in the midst of darkness. The writer's work just mentioned aimed, by illuminating this entire surrounding portion of the visual field to various brightnesses (stimulus intensities), to measure its effect upon the difference-threshold at the center. The fact brought out was the significant dependence of the value of $\Delta I/I_2$ within the central test-field upon the brightness of its independently controlled surroundings. When the latter was relatively high, and the test-field was relatively dark, the value of $\Delta I/I_2$ rose enormously; it reached a minimum when field and surroundings were of equal brightness, and rose again, by a moderate amount, as the surroundings were made relatively dark, to as near complete darkness as possible. In short, barring a small second-order difference which correlated with the all-around intensity of the total stimulus-complex, $\Delta I/I_2$, far from being constant, proved to be very definitely related to the intensity-ratio of test-field to surroundings.

We may grant then that Weber's 'law' is what it appears to be, namely, the statement of a special case; but this admission does not remove the necessity for examining an interpretation which has been put upon the difference-threshold. The experimental data could well be expressed in some general mathematical form other than equations (1), (2) or (3) above, such as

$$\frac{\Delta I}{f(I_1, I_2)} = c, \tag{4}$$

in which $f(I_1, I_2)$ increases with increase in the value of I_1, or of I_2 or both, but not necessarily in proportion. Although the subsequent mathematical operations and the result would be different, the assumptions involved in the implied interpretation are identical.

At this point it might be well to emphasize the sharp distinction that should be made between Weber's law and Fechner's interpretation of the difference-threshold. It so happened that Fechner initially applied his reasoning to the case of a difference-threshold which was assumed to obey Weber's law. The final conclusion, of course, depends upon

the validity of both, but neither one of the two stands or falls with the other. *The premises, on the one hand, and the mode of reasoning therefrom, on the other, are two distinct things, capable of separate consideration.*

The interpretation of the difference-threshold attributable to Fechner might be put as follows:

Sensation may be regarded as something having a magnitude S, which increases steadily with the magnitude I of the stimulus from which it arises. If under two conditions we find that the difference-thresholds are unequal, it means that equally noticeable, and hence equal, differences in sensation have resulted from unequal increments (or decrements) in stimulation. Sensitivity is therefore different in the two cases, and stands in reciprocal relation to the respective stimulus-differences required for equal sense-differences. Sensitivity could as well be measured as proportional to unequal sense-differences due to equal stimulus-differences, but as yet we have no means of appraising unequal differences in sensation.

According to Weber's law, the liminal difference ΔI is proportional to the stimulus-magnitude I, whence sensitivity will be as $1/I$ for any value of I. From this consideration Fechner writes the *Fundamentalformel:*

$$\frac{1}{I} \cdot dI = k \cdot dS, \tag{5}$$

dI and dS being very small concomitant differences in stimulus and sensation respectively, and k being a constant. This is to say, then, that the product of sensitivity into a very small increment in stimulus is proportional to the resulting increment in sensation.

Expression (5) is in form for integration with the result:

$$\log \frac{I}{I_0} = kS, \tag{6}$$

in which $\log I_0$ is the constant of integration and I_0 is the intensity of stimulus at which S (sensation) is zero. The conclusion reached is then: the magnitude S of any sensation is as the logarithm of the stimulus from which it arises.

Whatever favor this sort of interpretation of the facts may have enjoyed, it is certain that it has long met with indifference or disfavor in the minds of psychologists. In spite of that, at the present time one sees the formula (6), as it stands, brought into the literature (particularly of physiological optics) with the same assurance one would have in introducing a proposition from Euclid, and sees it similarly used, often as the foundation for extended mathematical work.[5]

Objections to Fechner's speculation were forthcoming as early as 1890, when William James discussed this subject in his *Principles of Psychology*. His objections, although largely metaphysical, are in part based on experimental data, and conclude with the statement that the outcome as to Fechner's theories is *nil*, and Weber's law alone remains.[6] The physiological texts as a rule, in connection with the discussion of Weber's law, mention Fechner's formulation. This is apparently done for the sake of completeness, as doubts are usually expressed as to its validity.

From equation (6), which affirms that the magnitude of the sensation is proportional to the logarithm of the stimulus-intensity, it would follow that any pair of stimuli bearing a given supraliminal intensity-ratio to each other should present an equal sense-difference, regardless of their absolute intensity. Thus, a pair of stimuli whose intensities are 10 and 11 should appear mutually different to the same degree as another pair whose intensities are 5 and 5.5, or 150 and 165. Hence if a person is required to adjust a third stimulus to an intensity at which it appears equally different from each of two unequal given stimuli, he should, if the logarithmic law holds, adjust it to the geometric mean of the intensities of the two. Experiments of this sort have been tried with various results.

[5] H. v. Helmholtz, Handbuch der physiologischen Optik, II. Aufl., pp. 409–414, and 444–470. Peddie, Colour vision, London, 1922, from p. 42 on, follows Helmholtz. I have some difficulty in interpreting the fact that Helmholtz's editors, in 1909, stepped over all this material when they selected the first edition for republication as the basis of the third. Many instances of the use of the *Massformel* may be found scattered through the literature.

[6] Wm. James, Principles of psychology, Henry Holt & Co., New York, 1890, Vol. I, p. 537 ff., and final paragraph of that chapter, p. 549.

Sometimes the logarithmic law is upheld, sometimes not.[7] It is to be noted, however, that since Weber's law has its important exceptions, such an experiment proves nothing at all for or against the reasoning of Fechner unless Weber's law has been verified by the original threshold method for the range of intensities involved, and with *all* other conditions identical with those of the check-experiment. On the other hand, a reliable calibration of the stimulus-scale in threshold steps, coupled with equally reliable observations, *under the same conditions*, as to the equality-of-difference of pairs of stimuli, paired to represent equal numbers of these steps, would constitute a check upon Fechner's reasoning, irrespective of Weber's law. While such an experiment would involve a large amount of labor, this is nothing compared with that which has been expended in futile discussions, metaphysical and otherwise, and in the rearing of mathematical superstructures upon dubious foundations; and the result will have infinitely more value. The validity of the assumptions upon which formula (5) is based cannot be tested speculatively. Their consequences must be put to the test of facts.

At the risk of needless repetition it is to be pointed out once more, that if Fechner's reasoning is to be confirmed by equation (6), it is necessary first that its premises, Weber's law, equation (3), be shown to be valid; and second, that the mathematical implications of equation (6) be shown to hold; and this, without the introduction into, or the removal from, the experimental situation in the mean time of any factor which would upset the relations implied in the premises (3).

For the cases in which Weber's law does not hold, a similar line of reasoning may equally well be applied, which begins with equation (4), and with corresponding mathematical treatment yields the general equation following for the value of the sensation-magnitude S:

$$S = F(I). \qquad (7)$$

Incidentally, it is to be remarked that all the necessary

[7] K. Dunlap, The elements of scientific psychology, Mosby, St. Louis, 1922, p. 128, and A system of psychology, Charles Scribner's Sons, New York, 1912, pp. 114–115.

mathematics may be carried out graphically in a comparatively simple way, thereby avoiding the difficulty of fitting a symbolic formula to the empirical data, and of the subsequent handling of what may be a very difficult expression.

The foregoing will constitute a general criterion of the validity of Fechner's interpretation of the difference-threshold. If the conclusions of Fechner's assumption coupled with the subsequent mathematical operations are borne out by experiment, we have a quantity S which fulfils just two conditions: (*a*) it differs by equal amounts, ΔS, when the two stimuli differ by a threshold amount, as defined by the method of threshold-measurement used; and (*b*) it differs by equal (larger) amounts, $n\Delta S$, in the case of any two pairs of stimuli which present equal apparent differences. Such are the strict mathematical implications of the quantity S, provided the two points above can be quantitatively verified by the corresponding experiments here described; if, on the other hand, the two conditions above prove to be at variance with experiment, Fechner's case collapses. The case will remain in the air, just as it has since Fechner's time, without the verification implied in condition (*b*). The fact that no exhaustive critical work has appeared upon this point to the present time may be due to one of two reasons: the results obtained have been indecisive or contradictory, and hence disappointing; or the subject has not carried enough interest to be an incentive to the necessary labor once in seventy years. Certainly it should not be said that the scientific fraternity has been wanting the necessary ingenuity and industry during the whole of this period.

I have concluded that the Fechnerian reasoning begins with an oversight; which for some unaccountable reason has escaped notice. At least it does not appear that any of the critics have given the point the emphasis which it deserves. *The fact overlooked is the fact that any two stimuli presented in conjunction will each modify the effect of the other.* 'Of course,' the loyal Fechnerian will say, 'one must make allowance for contrast.' On that point, then,we are in agreement. We will therefore proceed to make as much allowance for contrast as needs to be made.

Let us begin by selecting a standard stimulus *A*, and determine the value of a slightly greater stimulus *B*, such that the two differ by a threshold amount. We will assume that the experiment has been so conducted that all bias due to the spatial or temporal disposition of the two stimuli, or to other causes, has been eliminated. Then let us proceed by taking the stimulus of value *B*, so found, as the standard, and similarly determine the value of a third and still greater stimulus *C*, such that *B* and *C* again differ by a threshold amount. The Fechnerian assumption is then that the sensory effects of *A* and *C*, under the two different conditions, differ by two sensation-units such as are presented by the stimulus-pair *A–B*, or the pair *B–C*. This clearly rests upon the tacit assumption that the sensory effect of *B*-in-conjunction-with-*A* is identical with the sensory effect of *B*-in-conjunction-with-*C*. May this assumption be admitted? *The answer is no, it may not, if we are making allowance for contrast.*

For threshold differences, such as we are here dealing with, direct proof and disproof are both wanting on this point. On the other hand, what we do actually know about the mutual effect of stimuli presented in conjunction tends absolutely to disprove it. The facts are striking and unmistakable for the case of more-than-threshold differences. One need only mention, for the case of vision, simultaneous and successive contrast, to say nothing of adaptation and after-images, to be reminded of the total inapplicability of any statement which affects to express a value of sensation *S*, in terms solely of the magnitude of the stimulus *I* from which it arises, and in total disregard not only of the influence of the other compared stimulus, but also of all other stimuli to which the sense-organ is conjointly subject. A certain visual stimulus of moderate intensity may be made to appear glistening white or of the deepest black merely by presenting it in a setting whose photometric intensity is much less (dimmer) or much greater (brighter) than its own, and entirely without any corresponding change in the magnitude of the stimulus itself. What then becomes of the sensation-magnitude *S*, which is mathematically made to depend upon the stimulus-magnitude, but which

nevertheless goes through all of its paces, from one extreme to the other, with no change whatever in the stimulus-magnitude? The fact simply laughs in the face of any one who pretends to express the value of sensation in terms of a single stimulus-value. Indeed it is difficult to see how such an idea could be entertained at all, since the day when Hering made himself heard.

To revert to the three-stimulus paradigm, developed a moment ago: if the differences between *A* and *B*, and between *B* and *C*, are of more than threshold magnitude, it is a most striking fact that *B*-in-conjunction-with-*A* and *B*-in-conjunction-with-*C* do not present the same appearance, nor do they present anything that could by any stretch of the imagination be called like or equal sensations. Quite otherwise, as may be shown by a simple visual experiment which will be described, and which may easily be repeated.

Two fields are prepared by punching two round holes in a black card. In the actual model at hand these are 16 mm. in diameter, and the interval between them is 8½ mm. Of one of these, *L*, Fig. 1, one half (L_2) is covered with a microscopic

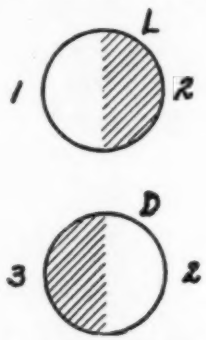

FIG. 1. The two fields *L* and *D* are identical in appearance as indicated by the shading; yet the right-hand halves L_2 and D_2 are identical, while the left halves L_1 and D_3 are two stages apart in photometric intensity.

cover-glass. The loss of light in its passage through this, about 8 percent, is sufficient to give the two half-fields a quite different appearance when viewed against a bright surface such as the sky. Of the other field *D*, one half (D_3) is also

covered in the same way, and in addition the whole field *D* is covered with a third cover-glass. If we assume that 0.92 is the transmission-factor of one layer of cover-glass, which is close to the fact, then will the half-fields L_1, L_2, D_2 and D_3 be related in brightness as 1.00, 0.92, 0.92 and 0.8464. According to the logarithmic law the two halves of *L* should appear different to exactly the same degree as the two halves of *D*. Inspection of the model against a uniform bright ground bears this out beautifully, for the two fields *L* and *D* are almost, if not quite, indistinguishable, as indicated by the shading in the figure. However, at this juncture it would be premature for us to throw up our hats and shout for Fechner and the logarithmic law.

What else is implied in the fact that the two fields are identical in appearance? Nothing but the fact that the half-fields L_2 and D_2, which are equal stimuli, and which should according to the logarithmic law appear identical, do in fact appear to be different, and this to the same extent as L_1 and D_3, which according to the logarithmic law should appear different by *two* stages, each equal to the difference in appearance presented by the two halves of either field. Thus for grosser stimulus-differences it is quite obvious that *B*-in-conjunction-with-*A* and *B*-in-conjunction-with-*C* do *not* result in equal sensory effects. How then can it be reasoned that the sense-difference *A* to *C* is the sum of the sense-differences *A* to *B* and *B* to *C*? The burden of proof would seem to rest upon Fechner's followers.

The difficulty in the way of the formulation of any one-to-one stimulus-sensation equation would seem to be the untenability of the view that the sense-organ is an instrument which, like a spring dynamometer, points to a different figure on its dial for every different load placed upon it, and which also always points to one and the same figure for equal loads. Rather must the sense-organ be looked upon as resembling a chemical balance, which gives only vague and uncertain indications of the total load placed upon its pans, but which nevertheless reacts with high precision to small differences between the two loads *by reason of the fact that the loads are acting in mutual opposition.*

It has been unwise, perhaps, to introduce the above analogy. Certainly it would be unwise to push it too far. Accordingly, it had better be abandoned at this point, in favor of a quantitative schema for the stimulus-sensation relationship outlined not long ago by Adams [8] and applied by him to the visual data cited above,[3] with a surprising degree of quantitative agreement. From certain assumptions, involving the all-or-none law of neural response, and also certain quantitative relations existing during the refractory period, a formula is derived which may be used to express the relation between 'sensation' S and the brightness B_1 of a visual stimulus:

$$S = k\frac{B_1}{B_1 + M}, \tag{8}$$

in which k is a constant and M is a term requiring some explanation.

In effect, this view of the sense of vision implies something which has long been an object of interest to psychology and physiology, and of surprise and curiosity to the laity. Numerous and often-repeated simple experiments have easily demonstrated the fact of contrast in obvious and striking ways, without at any time emphasizing the possibility that the phenomenon involved is much more than an 'optical illusion' or a parlor trick. In fact, these simple demonstrations of contrast, upon which Hering long ago laid such great emphasis in developing his theory of vision, are simply obvious appearances, under special and unusual conditions, of a general fact which is quite fundamental to the process of vision as a whole. *The retina is a compensating instrument.* There is no one-to-one relation between the stimulus-intensity at any one part of it and the magnitude of the consequent response within that part. On the contrary, the effect of the one stimulus is restricted, deflected or even reversed by the fact that other stimuli are acting in conjunction with it, either simultaneously at other parts of the retina, or within a limited time previously at any part. Unless these conditions are taken into account,

[8] E. Q. Adams & P. W. Cobb, The effect on foveal vision of bright (and dark) surroundings—V, *J. Exper. Psychol.*, 1922, 5, 39–45.

eliminated in some way, or equalized, no formulation of the stimulus-sensation relationship is even conceivably possible.

Accordingly, M appears in (8) in the denominator, by which fact it tends to offset the effect of B_1 upon the value of S; and further, M depends for its value not alone upon B_1, but also upon B_2, the intensity of the comparison-stimulus in the hypothetical threshold experiment, and upon other stimuli conjointly acting, to a greater degree the greater their proximity to the area occupied by the compared stimuli, and increasing in magnitude with increase in any or all of these. In short M is assumed to be some sort of a weighted mean of all stimuli acting at the time, or with sufficient recency to be effective, preponderantly depending upon those which are proximate in space and time rather than upon the remote ones, and possibly to a negligible degree upon some. In this way, the area of the retina which receives the test-stimulus is assumed to adopt a scale of response for the time being, and the measure of the scale depends upon M as defined.

Some of the consequences of this set of assumptions may now be discussed:

If the level of stimulation-intensity is increased by a certain factor n (as would happen in fact when the intensity of the light is increased by that factor *without change in its distribution*), then will B_1 and M both be changed by the same factor and the value of S will remain unchanged. Although this consequence is quite in accord with the implications of Weber's law, as will be pointed out later, it is hardly to be accepted as in complete accord with the facts, although it is nearly true as is evidenced by the unchanged appearance of objects under widely different illuminations. In all probability a compensating mechanism will not compensate completely, and it is better to assume that, for a wide range of intensities over which good vision is possible, M is not strictly proportional to the general level of stimulation but lags somewhat behind as the latter rises, being low relatively to the stimulation-intensity level in the upper part of this range, and relatively high in the 'dim' light at the lower.

How does this formulation square up with Weber's law? By differentiating equation (8) we arrive at the relation:

$$\frac{dB_1}{B_1} = \frac{1}{k}\left(\frac{B_1}{M} + 2 + \frac{M}{B_1}\right) dS, \tag{9}$$

from which it appears, assuming ΔS constant for the liminal difference, that $\Delta B_1/B_1$ should be minimum when $M = B_1$. Further, when $M = 3/2\ B_1$, $\Delta B_1/B_1$ rises to only 1.0425 its minimum value, which will obviously be exactly true again when $M = 2/3\ B_1$. Four percent in the magnitude of a threshold is by no means easy to determine experimentally. If we assume $M = 5/4\ B_1$ or $4/5\ B_1$, the value of $\Delta B_1/B_1$ will be only 1.0125 its minimum. Thus there is quite a wide range of discrepancy between the stimulus-magnitude and the value of the compensation-term within which the value of the relative differential threshold is not measurably changed, that is, within which Weber's law will hold true experimentally. Once outside these limits, however, Weber's law fails, and as a matter of fact it fails very fast at the two extremes of low and high intensity, when we may assume M to be initially ahead of the stimulation-level, or unable to keep pace with it, respectively.

As previously stated, this formulation has been applied [8] to the conditions of some early threshold measurements of the writer's,[3] which definitely disagree with Weber's law. By determining values for the constants, and an expression for M in terms of the intensities involved, it was possible to fit this formula to the data with surprising agreement.

Lastly, this formulation may be successfully applied to the result of the experiment with the two fields, L and D, described above.

If it is assumed that the minimum value of $\Delta B_1/B_1$ for the threshold is 0.01 when $\Delta S = 1$, the constant k in (9) becomes 400, and (8) becomes:

$$S = 400\frac{B_1}{B_1 + M}, \tag{10}$$

in which S is expressed in 'least perceptible differences' or threshold units. The intensities (which need only be rela-

tively stated) of the half-fields $L1$, $L2$, $D2$ and $D3$ are respectively as 1.00, 0.92, 0.92 and 0.8464. For M we take 0.96 (the exact mean) for field L, and 0.89 (slightly more than the mean) for D. This implies that the level of intensity of the field has advanced somewhat faster than the compensation-term M. We then have:

	Field L $M = 0.96$	Field D $M = 0.89$	Diff.
(1)	$S = 400\frac{1.00}{1.00 + .96}$	$S = 400\frac{.92}{.92 + .89}$	
	$= 204.1$	$= 203.3$	0.8
(2)	$S = 400\frac{.92}{.92 + .96}$	$S = 400\frac{.8464}{.8464 + .89}$	
	$= 195.7$	$= 195.0$	0.7
Diff.	8.4	8.3	

Thus the juxtaposed field-halves show a difference in either case, L or D, of 8.3; while the two fields as a whole differ by only 0.8, that is, a trifle less than a threshold. Moreover, $D2$ and $L2$, equal as stimuli, represent a sense-difference as large as 7.5, almost as large as any; while $L1$ and $D3$, two stages apart as stimuli, represent a sense-difference of 9.1, not much greater. These computed figures thus correlate in a striking way with what is observed on inspection of the model, described above.

The attempt to adapt a mathematical formula to biological data is often successful; the more so, the more limited the range of variables over which the data extend, and the more constant the secondary effective variables are held. Also, the attempt is more successful if the investigator is easily satisfied as to what constitutes a fit. In any case, data taken under other conditions are often disconcerting and ruin the theory. A complete mathematical description of the human sense of vision, verified by properly taken data at all stimulation intensities and under all conditions, is something which need not be looked for in the near future. Furthermore, beyond the *a priori* considerations upon which such formulation may be

based, there must be some demonstrable consequences of the theory upon which its empirical verification may rest, and which, at the same time, will constitute the foundation of its usefulness. In the language of the street: you can't prove it, and it doesn't mean anything anyway unless it makes a difference.

However, the formulation stated and discussed above follows the trend of the facts in a number of ways, and is better taken as a shorthand statement of a certain general characteristic of vision, and possibly of other special senses as well, which unfortunately was not adequately taken into account in Fechner's time, nor even to the present day. That characteristic is, in the sense of vision, a predominant tendency to ignore general stimulus-intensity and at the same time to retain a high grade of sensitivity to stimulus-differences. On teleological grounds, at least, a reason for this is not far to seek. The other senses react to stimuli which are inherently functions of the objects with which they originate, which objects are of biological importance to the organism. The touch of an object upon the skin, the taste of sugar, the odor of food, the horn of an approaching automobile represent physical stimuli each of which is characteristic of the thing which is its source.

On the other hand, visual objects, with few exceptions, are not seen by reason of any physical agent directly emitted by them. They are seen through the agency of light, which is characteristic neither of the object nor of the sense-organ, but which is an intermediary, a third party to the transaction, utilized because it is accidentally present. And under natural conditions this light is highly variable, somewhat as to color, but above all as to intensity. From the standpoint of evolutionary adaptation neither the color nor the intensity of the light has any survival value for the organism in so far as this depends upon his behavior toward the visual objects. Life is possible under all natural light-conditions, including darkness. The material object to be seen is the essential thing which determines the value of the sense-organ, and the intensity of the light is much better left out of account.

At any one time, the organs of vision take cognizance of a range of intensities of not more, perhaps, than one to one hundred. The eyes are, however, capable of working under intensities of light as low as one ten-thousandth of a foot-candle, and bright sunlight yields an illumination of ten thousand foot-candles. The actual working-range of intensity-levels for the eyes is therefore something like one to one hundred million. A comparison of these two ranges alone should suggest that the visual organs carry with them some compensatory mechanism, by which mere intensity of stimulation is made relatively unimportant, in order that *differences* in intensity, upon which the recognition of objects depends, may come to the front.

Doubtless, a compensatory shift in the scale of sensitivity is a characteristic of at least some of the other special senses. The familiar facts that the sugar in one's coffee loses its taste after one indulges in maple syrup, that an odor loses its effect when continuously inhaled, speak for the senses of taste and smell. The classical experiment in which tepid water may be felt as at once hot and cold speaks for the temperature sense. While such data may not immediately prove the point for the compensation theory, they certainly call for caution, in excess of that which has been exercised, in propounding a theory which proposes to link stimulus-intensity and sensation-magnitude in parallel scales, regardless of other circumstances.

We might for the present save ourselves much trouble by remembering that Weber's 'law' is an exception, and by looking for empirical verification, not of Fechner's *Massformel* (the so-called logarithmic law), but of the method of reasoning by which he arrived at it.

Summary

1. The questions involved in Fechner's *Massformel*, ostensibly based on Weber's law, are reopened.

2. In giving mathematical conciseness to the statement of Weber's law, the meaning of the threshold is incidentally discussed; and the fact is pointed out that from the assumption of the exact truth of Weber's law, it follows mathematically that there can be no absolute threshold.

3. Attention is called to the fact that Weber's law may be considered no more than the approximate statement of a special case. Experiment has shown that it has no universal validity.

4. It is shown that there is an error in tacitly assuming that Weber's law and Fechner's logarithmic formulation are one and the same thing, and that they must stand or fall together; and it is further shown that Fechner's reasoning might equally well be applied to *any* statement of the magnitude of the difference-threshold.

5. Fechner's derivation of the *Massformel* from the statement of Weber's law is reviewed, some early objections are discussed, and the procedure for an experimental check on Fechner's reasoning, independent of any assumption as to Weber's law, is indicated.

6. A fallacy which appears to have been overlooked is pointed out and illustrated by a simple experiment. This is, briefly, the failure to take into account the facts of contrast, which at once invalidate any attempt to formulate sensation-magnitude in terms of the value of a single stimulus from which such sensation is held to be derived.

7. For the case of a variable stimulus, presented in surroundings which themselves constitute an invariable stimulus, in which case Weber's law does not hold at all, a formula which is totally different from Fechner's *Massformel* fits the experimental facts tolerably well.

8. The functions of the organs of vision, and of the other special senses as well, will be better understood if the fact is realized that, on the one hand, the mere intensity-magnitude of the stimulus is often of little importance while, on the other, between stimuli juxtaposed in space or time, *difference* in magnitude is paramount.

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SCIENTIFIC UNDERSTANDING IN PSYCHOLOGY [1]

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Human nature as portrayed by the novelist is radically different from the psychology of the textbooks. The writer of so-called psychological novels may be utterly ignorant of the basic material introduced into our elementary courses and nevertheless handle his characters with a deftness and penetrating insight which causes us, in our non-professional moments, to agree that he 'understands' human nature. Using the term with this connotation it may be doubted whether the most distinguished psychologists surpass the novelist in understanding man's behavior.

Rigorous training and exceptional competence in academic psychology may exercise little or no appreciable influence in the direction of making the student more expert in dealing with people. His capacity to understand them and to appreciate their routine problems and everyday difficulties is not noticeably enhanced by his formal psychological studies. The Phi-gamma hypothesis, eidetic imagery, correlation ratios, the two-factor theory of intelligence and kindred fruits of psychological erudition fail to give him much more insight into the 'nature' of his landlady or his parents than his brother specializing in philosophy or physiology. It is not at all unusual for an alert police officer, a wide-awake business man, a successful physician, or a keen head-water to 'know' more about human nature than he does. And yet he is supposed to be the expert in this kind of nature. Is there something wrong with his training or are there two kinds of human nature: the kind described in our textbooks and the kind depicted in the novel or on the stage?

[1] This article is an outgrowth of the writer's work at the University of Vienna as a fellow of the Social Science Research Council during 1930–31. He desires to take this means of acknowledging his indebtedness to the Council for its generosity in affording him the opportunity of devoting himself to the study and reflection upon which this analysis is based.

The implications of this problem go beyond the horizons of psychology viewed as an independent, self-sufficient study. To the extent that the social sciences in general look to psychology for interpretative, conceptual, and methodological enlightenment we have such transcendent implications. History, government, sociology, economics and allied fields of investigation whose major interests pivot around the nature of man are forced to pursue these investigations with pathetically meager reliance on the theoretic and factual products of the psychological laboratories. The latter products for the most part are too remote from the mental realities which the penologist is striving to understand and control. An historian confronted with the task of revealing the mental processes responsible for the rise of a Napolean or the fall of a Caesar would secure very little in the way of relevant clues from the pages of our standard textbooks of psychology.[2] Such texts viewed as propædeutic and ancillary to the other social sciences in the sense in which physiology is related to the medical sciences are most disappointing. Dilthey recognized this years ago and his controversy with Ebbinghaus (**6**) was the consequence of this recognition. Broadly speaking, Dilthey's proposed solution of the difficulty called for a rather rigid bifurcation within the stream of psychological progress. Traditional psychology as developed by Wundt and his laboratory contemporaries with its preoccupation with the concepts and methods of the natural sciences was to represent one branch. The other branch was to give rise to a psychology rooted not in the natural sciences, but in the problems,

[2] In connection with his discussion of the dependence of historical research upon 'psychic facts' Windelband introduces the following challenging footnote:

"But we must again emphasize that it by no means follows from this that, as has often been said and still more often thoughtlessly repeated, psychology is the foundational science of all historical culture. This is not at all true of scientific psychology, which as to its method belongs to the natural sciences, and in its content is an inquiry, apart from value, into the uniform movements of the psychic elements. Its theories are no nearer to the interest of historical research than those of other sciences are. The psychology which the historian uses is a very different thing. It is the psychology of daily life: the practical psychology of a knowledge and understanding of men, the psychology of the poet and the great statesman—the psychology that cannot be taught and learned, but is a gift of intuitive intelligence, and in its highest form a genius for judging contemporary life and posterity. This sort of psychology is an art, not a science." (17, p. 280.)

methods, and modes of thinking germane to history and biography. What was proposed was an antithesis between *naturwissenschaftliche Psychologie* on the one hand, and *geisteswissenschaftliche Psychologie* on the other.

The latter term possesses no precise English equivalent. Since its connotation is not unlike the word 'kulturwissenschaftlich' used to designate the philosophic movement represented by such men as Windelband and Rickert its meaning may be indicated by the phrase 'cultural science psychology.'[3] By way of further orientation it might prove helpful to consider the discussion of the term *Geisteswissenschaft* as treated by Apel (1, pp. 51–52). Paraphrasing his account rather closely, we may say that the *Geisteswissenschaften* include those sciences which study the influence of the state, society, law, custom, education, house-keeping, and the technical sciences on the business of living, as well as those sciences concerned with the interpretations of the world as revealed in language, myth, art, religion, philosophy and science. The differentiation of the latter group of sciences from the group embraced by the term natural sciences goes back to Bentham, Ampere, Mill, and Hegel. However, not until about the middle of the nineteenth century did the expression *Geisteswissenschaft* attain a definite stamp as a result of Dilthey's efforts. For him this expression referred to "the totality of sciences whose sphere of investigation is historico-social reality"; and whose task it is "to re-live and thoughtfully to comprehend" the manifestation of this reality.[4] Wundt sees a task for the *Geisteswissenschaften* wherever "man as a volitional and cognitive subject constitutes an essential factor in the phenomena" under consideration.

The content of all the *Geisteswissenschaften* is immediate experience in so far as that experience is determined by the interaction between objects and discerning and manipulating subjects. As a consequence these sciences do not avail them-

[3] See Klüver's excellent summary of some of the main implications of this type of psychology in the last chapter of the text by Murphy (13).

[4] The quotations are verbatim translations of original citations from Dilthey's works introduced by Apel. The same applies to the quotation from Wundt immediately following.

selves of the abstractions and the hypothetical conceptual aids of the natural sciences. What they take as their immediate reality are, rather conceptual objects (Vorstellungsobjekte) plus the inner or subjective activities accompanying such objects. Psychology is to be basic for the *Geisteswissenschaften.* In terms of Windelband's distinction the opposition between the two types of sciences may be indicated by characterizing the latter group as *Ereigniswissenschaften* in contradistinction to the *Gesetzwissenschaften.* By this he means that natural sciences are primarily concerned with getting at the *laws* back of phenomenal events. In his terminology they are *nomothetic.* The *Geisteswissenschaften,* on the other hand, are *idiographic* or primarily concerned with the complete and exhaustive presentation of a *single* happening, as illustrated by the science of history. In investigating nature we seek laws, in history we seek forms.[5] For the nomothetic or natural sciences the *single* happening or isolated given object serves only as a type, as a special instance of a generic concept. The historian's task, however, is to take a realm of the past strongly marked as it is by its own characteristics and to enliven it in such a way that ideationally it becomes a living present. Nomothetic thinking veers to the abstract, while the thinking of the historian tends more to the evident and observable.[6] It is ideographic.

Rickert's philosophy of history furnishes an excellent means not only of clarifying these distinctions, but also of focalizing the issues under consideration. The ordinary, unreflective view which regards history as the record of past events is seen to be descriptively vague and inaccurate as soon as the historian's activity is observed more reflectively and critically. Not every past event is historical. To take a hypothetical case: if an old letter reveals that John Jones, an obscure clerk of an obscure firm, spilled ink on his clothes on April 13, 1803, the mishap in question does not necessarily interest the historian. Neither is he apt to be interested to

[5] As Apel formulates it: "Naturforschung sucht Gesetze, die Geschichte Gestalten."

[6] "Das naturwissenschaftliche Denken neigt zur Abstraktion, das historische zur Anschaulichkeit."

learn which members of the House of Lords during the reign of Edward the Seventh preferred having their steaks well done. Of the infinite multiplicity and variety of events only relatively few merit the designation of historical and the criterion of such merit is only incidentally and rather irrelevantly bound up with the temporal locus of the event.

Sheer pastness as such does not automatically endow an event with historic relevance. The writing of history requires criteria of selection by means of which such relevance may be determined. These criteria, according to Rickert, are a function of the processes responsible for the rise and development of civilization and culture. In orienting himself with reference to the ceaseless flux of countless happenings the historian utilizes the criterion of cultural significance as a selective and guiding principle. Cultural values as exemplified in the bearing of given events on the evolution of commerce, science, art, law, morality and religion serve to differentiate the historically essential from the historically trivial and non-essential. History is thus concerned with the particular to the extent that it articulates with such values.

These values as differentiating and descriptive categories are characteristic not only of history, but of all the cultural sciences or *Geisteswissenschaften.* Viewed from this angle we have one of the most decisive respects in which the latter differ from the natural sciences. Physics and chemistry deal with the particular as a means of approach to those 'value-free' abstractions designated as 'natural' laws. The particular as such is inconsequential, whereas it may be momentous for the *Geisteswissenschaften.* For the petrologist the Rosetta stone would be just another stone, but for the 'ideographer' it possesses features of uniqueness incapable of substitution. He could not regard it as just another stone.

It should be obvious that we are not wrestling with elusive metaphysical phantasms, but with phenomenal processes and events susceptible of experience and control. In fact, as Messer (11, p. 138) points out, Rickert repudiates both metaphysics and a transcendental reality. He is merely concerned with the task of demonstrating the limits of the

physico-chemical,—using this term as synonymous with *naturwissenschaftlich*,—description of our phenomenal world. Such a description even when carried to ideal completion would still leave scientifically important questions unanswered.

To avoid misunderstanding, even if it involves something in the way of a digression, it might be well at this point to call attention to a frequently neglected ambiguity resident in the phrase *physico-chemical description*. This phrase may refer either to a metaphysical conviction or to a methodological program. When one says man is ultimately, exclusively, and exhaustively reducible to electron-proton units he is giving expression to a metaphysical as opposed to a scientific formulation. On the other hand the endeavor to describe man in terms of physical and chemical descriptive categories is merely *one* of several possible *scientific* methodological procedures. He can also be described in terms of economic, geographic, historical, linguistic, æsthetic, ethical and other descriptive categories. These categories are just as legitimate and 'scientific' as the physico-chemical and much more adequate and appropriate for many types of problems. Would the economist improve the scientific status of his work if he were to seek a chemical formula or a set of physical constants by means of which to specify the concept of a protective tariff?

To elevate the physico-chemical descriptive venture above the others as fundamentally more 'scientific' is to confuse the metaphysical with the methodological. Science is a matter of *method* and not of *Weltanschauung*. The essence of this method is the search for negative instances,—or in the case of experimentation, the control experiment—coupled with an attitude of unswerving allegiance to the *facts* irrespective of their metaphysical, systematic, or personal implications. This is the *sine qua non* of scientific work. Without it our work sinks to the level of the propagandist, the special pleader, or the debater who is more interested in being on the 'winning' side than on the right side. That is why the court-room lawyer is not to be classified as a scientist while the competent

specialist in comparative jurisprudence merits such classification.

It may well be that the ambiguous connotation of the term *physico-chemical* which we have just pointed out is responsible for some of our current controversy in psychology. The aversion manifested by some contemporary psychologists with reference to what they more or less contemptuously label 'conscious states' may partially be due to the ambiguity in question. Striving to give psychology the stamp of scientific respectability they are led astray by the real or fancied *metaphysical* implications of the physico-chemical technique of investigation. Aspirations, thoughts, pains, and disappointments with their characteristic experiential features are either symbolized by their physiological concomitants or else translated into the language of a hypothetical physiology.[7]

[7] The following quotation which we came across after writing the above passage may prove illuminating. It is taken from the work of the psychopathologist Jaspers, well known for his efforts to introduce the viewpoint of 'verstehende' psychology into psychopathology. In a section headed 'Die Vorurteile in der Psychopathologie,' Jaspers writes:

"Das noch immer verbreitete 'somatische Vorurteil' lautet etwa: alles Seelische ist als solches gar nicht zu untersuchen, es ist blos subjektiv. Soweit von ihm wissenschaftlich geredet werden soll, muss es anatomisch, körperlich, als körperliche Funktion vorgestellt werden; hiefür sei es immerhin besser eine vorläufige anatomische Konstruktion, die als heuristisch gilt, zu besitzen als eine direkte psychologische Untersuchung. Solche anatomischen Konstruktionen sind durchaus phantastisch ausgefallen (Meynert, Wernicke) und werden mit Recht 'Hirnmythologien' genannt. Dinge, die gar keine Beziehung zueinander haben wie Rindenzelle und Erinnerungsbild, Hirnfaser und psychologische Assoziation werden zusammengebracht. Es fehlt für diese Mythologien auch insofern jede Grundlage, als nicht ein einziger bestimmter Hirnvorgang bekannt ist, den einem bestimmten seelischen Vorgang als direkte Parallelerscheinung zugeordnet wäre. Die Lokalisation der verschiedenen Sinnesgebiete auf die Hirnrinde, der Aphasien an die linke Hemisphäre bedeuten nur, dass diese Organe intakt sein müssen, damit ein bestimmter seelischer Vorgang möglich sei; jedoch im Prinzip nicht anders als auch das intakte Funktionieren des Auges, der motorischen Mechanismen usw. dazu notwendige Werkzeuge sind. Man ist in den neurologischen Mechanismen weiter hinauf gedrungen, aber von den dem Seelischen eventuell parallel gehenden Erscheinungen unendlich weit entfernt. Die Frage, ob Seelisches und Körperliches im Parallelismus oder Wechselwirkung stehen, ist daher empirisch gar nicht zu entscheiden und für uns gänzlich uninteressant. Wir kennen keinen einzigen Fall, in dem wir empirisch das eine oder das andere konstatieren könnten. Das Seelische und die uns zugängliche körperliche Phänomene sind durch einen unendlichen Bezirk von dazwischenliegenden Phänomene, die wir nicht kennen, getrennt. Wir können in praxis sowohl in der Ausdrucksweise des Parallelismus wie in dem der Wechselwirkung—tatsächlich meist in der letzeren—sprechen. Dies können wir um so mehr, als es jeden Augenblick gelingt, die eine in die andere Ausdrucksweise zu übersetzen. Was aber die Neigung angeht, das Psychologische in somatische Vorgänge phantastischer oder realer Art zu übersetzen, gilt zu Recht was Janet sagt: wenn man immer anatomisch denken muss, muss man resignieren und nichts denken, wenn es sich um Psychiatrie handelt" (10, pp. 13–14).

It should be noted that description *sui generis* is sedulously avoided. Something akin to what may be called a dualistic phobia impels these investigators to avoid reference to psychological phenomena not readily transposable into physico-chemical concepts. Hence the curious anomaly: vague and utterly hypothetical physiological processes are substituted for clear and unambiguously real and existent conscious processes. To regard physiological concepts as more 'scientific' than psychological ones can be justified only in terms of the metaphysical preoccupation under consideration. It is this dualistic phobia which makes 'synaptic resistances' seem more 'real' than ideals or visual images. In fact, this dualistic phobia may be so strong that such images may actually be referred to as 'ghosts.' Pathetically enough, the very psychologist making a reference of this kind is the one to admonish his fellow-psychologists to purge themselves of all metaphysical toxins clogging their scientific machinery.

The admonition actually pleas for the acceptance of one form of metaphysics. This plea, of course, does not receive direct formulation, but by implication is assuredly resident within the polemic. An entirely laudable attempt to fortify the methodology of psychology is thus vitiated by the failure to note the ambiguity lurking in the phrase 'physico-chemical' description. The metaphysical connotation is confused with methodological. As a consequence, the resultant diatribe against the alleged metaphysical halo presumably responsible for the supposed shortcomings of non-behavioristic psychology strikes us as decidedly ironic. For a not very different reason the quasi-theological denunciation of behaviorism as 'materialistic' irritates us. To label *scientific* programs as spiritualistic, materialistic, idealistic, or realistic may be a fascinating metaphysical pursuit, but is a barren and futile endeavor for the scientist. The injection of such metaphysical issues into scientific thinking is of value only to the extent that they give rise to the formulation either of verifiable or of heuristic concepts.

However, so far as we know, the introduction of metaphysical concepts has rarely been the means of fructifying a

science, while, on the contrary, their elimination has often exercised an accelerating influence on scientific progress. It is unfortunate, but historically understandable, that preoccupation with metaphysical problems is more prevalent among psychologists than among other scientists. Our excursion into this field of thinking is only justified by this prevalence; for, as we see it, once psychologists succeed in liberating themselves from their metaphysical shackles a not inconsiderable portion of current misunderstanding and controversy will be dissipated. As should be evident, in adopting this stand we are merely reiterating the general principles of Comte, Mach, Avenarius, Wundt and of the positivist movement within modern philosophy which they initiated. Such reiteration strikes us as called for at this time because the failure to abide by these principles continues to obstruct psychological progress despite the implicit or avowed recognition of these principles by most psychologists.

Incidentally, if our analysis of the situation is sound, the failure in question is in part due to the positivists themselves in the sense that their disposition of metaphysical issues was rather cavalierly negative. By ruling such issues out of *scientific* court they were theoretically eliminated, but by no means annihilated. The consequence is they keep bobbing up in scientific discussions in more or less disguised forms like ineffectively repressed urges as pictured by the psychoanalysts.

To say that metaphysical questions are not susceptible of scientific formulation and hence are incapable of solution by the methods of science strikes us as a valid contention. However, if, as the positivists imply, this means that these questions should receive no sustained recognition at all, a dangerous *non sequitur* is in the offing. This danger threatens psychology especially because of the great ease with which its subject matter can be converted into grist for the metaphysical mill. Psychology can be protected against this danger by the simple device of having man's irrepressible interest in metaphysics taken care of by the philosophers. In striving to legislate such an interest out of existence the

positivists, it seems to us, are running counter to an inalienable characteristic of human nature. Even their anti-metaphysical bias may be construed as a product of their metaphysical interest.

The particular brand of metaphysics espoused by a given individual is in part due to temperamental factors and this very likely accounts for some of the diversity of opinion among metaphysicians and may even mean there can never be complete agreement. But this does not mean, as the positivists seem to think it means, that philosophy should cease to concern itself with such matters. The thinking individual seeks an outlet and a basis for orientation with reference to his metaphysical bepuzzlements. It should continue to be the task of the philosopher to guide him through the various options that confront him at the metaphysical crossroads. It isn't so much a question of converting him to one brand of metaphysics as opposed to another, as it is a question of introducing him to the available brands. He can then make a selection on the basis of his personal temperamental needs and if his needs are akin to those of the positivists, he can refuse to make a selection. We have changed our metaphor to emphasize the fact that such orientation does not call for high pressure salesmanship. On the contrary, it calls for impartial presentation of the pros and cons clustering around each traditional metaphysical approach with relative indifference concerning the final choice.

If our contention is valid, the positivists are thus partly to blame for the continued muddle between psychology as science and psychology as metaphysics. Parenthetically, we might add that Watson's prophecy of doom concerning philosophy because of its metaphysical interests is not only ludicrous in the light of the *metaphysical* arguments he adduces in support of radical behaviorism, but also short-sighted in the light of a vision purporting to point the way for the progress of scientific psychology. As we have endeavored to indicate, unless philosophy continues to evaluate the metaphysical implications of the special sciences in general and of psychology in particular, they will continue to obtrude themselves

in a variety of forms to the confusion of the sciences in question. It may not be very much of an exaggeration to say, Watson and the positivists to the contrary notwithstanding, that it might aid scientific progress if the scientists knew more metaphysics. Such knowledge would at least increase the chance of their differentiating metaphysical from scientific issues and interpretations.

To a certain extent allegiance to the metaphysical implications of physics and chemistry is also responsible for another source of inhibition and confusion in modern psychology. In some respects this source of trouble is even more in need of diagnosis and treatment than the difficulties we have just mentioned. Dilthey's proposed bifurcation within the stream of psychological development leading to the natural science type of psychology on the one hand and the *geisteswissenschaftliche* type on the other hand may have been due to this outgrowth of the physico-chemical metaphysical bias. We refer to the problem of what constitutes scientific understanding. This is a central but somewhat neglected problem. Occasionally of late, in psychological literature the problem has been alluded to in a rather superficial fashion. As an instance of this treatment we may consider the following quotation from Bretscher's (3, p. 24) recent article:

"As yet, the old saying of Lord Kelvin holds true: 'If I can make a mechanical model, I can understand it. So long as I cannot make a mechanical model all the way through, I cannot understand it.' This intellectualization may utterly falsify and distort reality. Nevertheless, it is the *only scientific method*[8] which we poor mortals possess."

It may prove instructive to consider whether Lord Kelvin's strictures on understanding hold good for science in general and for psychology in particular.

To limit human understanding to situations capable of mechanical representation strikes us as an indefensible contention. There are innumerable instances of relatively thoroughgoing understanding which are utterly refractory to such

[8] Italics ours.

representation.[9] We can understand mathematical relationships, the desperation of jobless men, the joy of scientific discovery, or the shortcomings of a constitutional amendment without having to resort to mechanical models. Similarly we can understand how England's embargo forced Germany to terms even though construction of a mechanical model of the events in question would assuredly be beyond our understanding. There is no need to press so obvious an issue.

What may not be so obvious is a classification of the ways in which we 'poor mortals' understand things. Upon reflection it seems to us that any given instance of understanding may be classified under one of more of four different categories. By way of preliminary orientation these categories may be listed as four types in the following fashion:

1. The structural continuity type;
2. The functional type;
3. The logical or implicative type; and
4. The psychological or empathic type.

Taking these up in order, we have first the type of understanding capable of diagrammatic treatment or of exposition by means of mechanical models. Whenever we deal with processes necessitating continuity of structure to guarantee adequacy of function we are dealing with this type. As a convenient designation we have labelled it the structural continuity type. To understand the reason for darkness when the electric light circuit is broken by a burnt-out bulb, or to understand the nature of heteronymous hemianopsia because of a lesion affecting the chiasm would be examples of this type. To understand how a crowbar may function as

[9] The statement is qualified by the word *relatively* because of the inherent complexity of the factors involved in the concept of understanding. Among the most troublesome of these factors are those having to do with the nature of causality. To discuss the concept of cause would not be sufficiently relevant to our present purpose. Furthermore, even if it were relevant, we lack the qualifications and insight needed to dispose of so knotty a problem. For a casual treatment of the problem in recent psychological literature see Hollingworth's text on abnormal psychology (**8**, pp. 69–71). A more detailed analysis from the point of view of logic is to be found in the article by Mitchell (**12**) cited in our bibliography.

a lever would be another example.[10] This type of understanding is akin to what Holt (9, pp. 157–163) refers to as the 'bead theory' of causation. Pushing the initial bead of a string will cause the terminal bead to move because of the successive contacts of the intervening beads. Continuity of structure presumably enables us to understand continuity of function.

But how does understanding occur in the absence of such structural continuity? How shall we understand gravitation or the propagation of light? At one time the physicist felt constrained to posit such continuity in order to circumvent the difficulty of accounting for action at a distance.[11] To accomplish this purpose he resorted to the fiction of ether conceived of as an elastic solid. The modern physicist, however, seems to have dispensed with the mythical ether and to have substituted a functional concept in place of it. To the extent that such functional concepts enable us to understand gravitation and related phenomena we may regard them as instances of the functional type of understanding,—our second category.[12]

A few specific examples may facilitate comprehension of the nature of this category. In this connection it might be well to note that mechanical models would not aid us very much in understanding the fall of a baseball dropped from the top of the Washington monument or the transmission of wireless signals across the Atlantic or the phenomenon of X-rays shooting across a vacuum. We are confronted with the necessity of adjusting our conceptual thinking to phenomena characterized by measurable and predictable changes of a functional sort in the absence of any observable or inferential structures to mediate the functions in question. To grasp the electrical theory of matter involves the ability to

[10] For an elementary analysis of the complexities underlying even this type of understanding see Herrick. Our illustration of the crowbar is taken from his work (7, pp. 35–37).

[11] A concise and simple sketch of some of these problems in the history of physics is to be found in the very readable little book by Sullivan (15).

[12] These categories are by no means mutually exclusive. For example, understanding the dynamics of the nervous system in terms of the *Resonanztheorie* of Paul Weiss (16) involves a fusion of our first two categories and possibly the third as well.

understand in the sense of this second category. The concepts of space and time as well as some types of functional neuroses involve this type of understanding. A similar statement may be made concerning many of our chemical concepts.

The third type of understanding can be illustrated by any instance of syllogistic reasoning. When we hear that all men are liars, we understand the mendaciousness of any particular man. This is the logical or implicative type of understanding. Without it mathematics would be bankrupt and the most ingenious mechanical models would be futile as substitutes. Denial of the validity of this type of understanding would cripple all of the other sciences as well in so far as they resort to logical inference in the prosecution of their problems. Astronomical prediction, medical prognosis, the search for unknown chemical elements on the basis of periodic tables represent examples of scientific work rooted in this third category. In fact, the anticipation of the consequences of any working hypothesis is largely a function of this sort of understanding.

Less abstract than the foregoing type is the last which we have termed psychological or empathic understanding. Here we have to do with the task of fathoming human motives and appreciating the entire gamut of human desires. To understand the foot-ball player struggling for a touchdown, the philologist tracking down an obscure root, the housewife bargaining with the vegetable dealer, or a ward politician truckling to the newspaper men requires neither mechanical models, a knowledge of functional relationships nor expert mastery of logical implications. But it does require a knowledge of human nature. It represents the type of understanding indispensable for the development of psychology as a social science or as a *Geisteswissenschaft.*

So long as psychologists continue to be inhibited in their professional work by the fear of not being utterly orthodox in their acceptance of what we have designated as the metaphysical connotation of the physico-chemical ambiguity, the development of psychology as *Geisteswissenschaft* is apt

to make little progress. The first three categories of understanding have firmly entrenched themselves as integral parts of psychological thinking just as they have in other sciences. But the fourth, which should be unique to psychology, has been seriously neglected.

There are numerous reasons for this in addition to those we have mentioned. For one thing the force of scientific tradition—particularly laboratory science—has been against it. Empathic understanding preceded the other types in the history of the race. Animism, astrology, alchemy, primitive religion and the vast realm of superstition and mystical taboo out of which modern science so painfully emerged was shot through and through with this type of understanding. Before astronomy, physics, and chemistry could attain scientific status the anthropomorphic features of their historically antecedent pseudo-sciences had to be eliminated. It would be tragic if they were permitted to return. The natural sciences have no business with our fourth type of understanding. For the modern chemist to speak of elective affinities or for the meteorologist to ascribe anger to thunder would be not only a 'pathetic fallacy,' but a reversion to prescientific thinking.

However, if our interpretation is sound, the psychologist made a tactical blunder in his efforts to place the study of mental phenomena on a scientific basis. He followed the pattern of the natural sciences too slavishly. He seems to have reasoned that since it was necessary for these sciences to purge themselves of all vestiges of anthropomorphism his science must do the same. Although it may sound heretical, we feel constrained to say that this is precisely what he should not have done. He alone was justified in making his science anthropomorphic. To argue that the study of human nature must be devoid of anthropomorphic interpretations is akin to urging the chemist to steer clear of chemical interpretations.

For fear of having this seemingly heretical opinion stigmatized as a plea for a return to mysticism or animism we hasten to register an emphatic denial of any such intention. We

are merely pleading for a program of courageous allegiance to experiential phenomena *sui generis* irrespective of what metaphysicians may do with such phenomena. Science as such must be indifferent to the metaphysical consequences of its findings.

Incidentally, we deem it advisable to anticipate another possible misinterpretation of what we have said. In our analysis of the four types of understanding, which should be regarded as nothing more than a first approximation to a solution of a complex and difficult problem, it was not our intention to suggest, even by implication, that psychology must avoid the first three categories. On the contrary psychology and all of the social sciences ought to utilize them freely and unreservedly in every situation to which they are relevant. In addition, though,—and this is our main point—the *Geisteswissenschaften* have a need and a right to avail themselves of the fourth type, even though the natural sciences must under no circumstances permit empathic interpretations in their respective fields. In other words, psychology involves all four categories, while the natural sciences involve but three. To be scientific, psychology must become more anthropomorphic, not less so, paradoxical and tautological as this may sound.

Although urging the desirability of making psychology more of a *Geisteswissenschaft*, we are, of course, not pleading for the establishment of another school or system of psychology. Such separatist movements are to be deprecated. We are merely advocating the recognition of a hitherto neglected aspect of the main stream of traditional eclectic psychology. In saying this we do not wish to imply that scientific psychology has *utterly* neglected the *geisteswissenschaftliche* approach. But we do feel the need for making this approach more focal than has been the case in the past.[13]

[13] The approach in question is allied to Spranger's *verstehende Psychologie*, but we hesitate to endorse his type of psychology. Apart from a quasi-mystical atmosphere which he injects into his system, his exclusion of physiological factors from the field of psychology as well as the completely non-experimental nature of his work render it of dubious value, even though much of it is brilliant and stimulating. He seems disposed to restrict his psychology to what we have called empathic understanding.

In its struggle to become empirical and experimental psychology has tended to pattern itself after the dominant science of the period. Borrowing an idea from Brett (4, p. 204), we might recall Hobbes' attempt to reduce mental life to movement following Galileo's conspicuous success in studying motion. "Locke and Hume suffered from excessive respect for Newton and gravitation; Hartley seemed to think that the secret of the Universe could be solved in terms of sound." As Boring (2, p. 208) has recently put it, when we come to the nineteenth century we find James Mill standing "for a 'mental mechanics,' just as his son, John Stuart Mill, represents 'mental chemistry.'" We then find Bain basing his psychology on the findings of the physiologists. With the rise of modern biology we find more and more psychologists introducing biological concepts into their psychology. All of this tended to make for the 'natural science' type of psychology, and on the whole, from the vantage point of historical perspective, can be approved of as necessary steps in the direction of rendering psychology a science. But we are now in the twentieth century and it seems to us the time has come for still further development. So far as the history of science is concerned this century is conspicuous for the emergence of the social sciences. It seems to us it will make for the continued progress of psychology if it follows its historic precedent, and proceeds to adjust itself to the findings, concepts and needs of the social sciences. We have endeavored to show there is no scientific disgrace involved in taking such a step. In fact, we venture to predict that once psychology succeeds in becoming more of a *geisteswissenschaftliche* science

Our three other varieties of understanding would hardly fit into his system. As an instance of this trend we might very briefly consider the way in which Spranger handles the question of mental changes taking place during puberty. He asks:

"Wird mir die grosse seelische Veränderung die beim Übergang aus dem Kindesalter in das Pubertätsalter vor sich geht, irgendwie psychologisch klarer dadurch, dass bestimmte Drüsen eine verstärkte Tätigkeit entfalten oder nicht entfalten? Diese Erklärung leistet ebensoviel wie die Behauptung, Sokrates sitze deshalb im Gefängnis, weil er seine Beinmuskeln bewegt habe und auf die Art hineingekommen sei" (14, p. 24).

The one-sidedness of Spranger's type of psychology has been pointed out by Karl Bühler (5). He also takes up the implications for scientific psychology of the issues Spranger raises in the passage we have just quoted (5, pp. 152–161).

it will gain increased respect not only from the social sciences but from the natural sciences as well.

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PSYCHOLOGY OF FEELINGS AND EMOTIONS

I. THEORY OF FEELINGS

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Two contributions to the theory of emotions have recently appeared in psychological literature; and, although these are antagonistic in point of view, each assumes the existence of actual emotional patterns differentiable in terms both of introspection and of reaction tendencies, without considering fully the implications of such a position or attempting to integrate all the experimental data.

Any projected theory of the emotional response must include, and satisfy the requirements imposed by, the following sets of data: (1) behavioristic: facts on emotional behavior, expression, smooth muscle activity, etc.; (2) introspective: the conscious attributes of emotion, its *quale* and concomitants; (3) psychopathological: cases of abnormal behavior and emotional experience; and (4) physiological: the data on physiological and neurological functions involved in the emotional response. Any theory which disregards any of these four fields must inevitably be inadequate.

The James-Lange hypothesis, defended by Newman, Perkins and Wheeler (**20**), is weak in several of these respects, but its deficiency is most notable in the field of psychopathology and physiology. It is therefore natural that Cannon, who in his recent article (**7**) defends the Cannon-Dana theory, should feel somewhat unkindly toward this point of view. The latter theory, on the other hand, neglects certain important facts in the behavioristic and introspective data. Extremely psychological psychologists, therefore, are quite unfavorable to the diencephalic hypothesis.

The modern trend of experimental work indicates that no behavior patterns can be identified as occurring concomitantly

with the introspective report of an emotion.[1] Behavioristically emotions are not satisfactorily distinguishable. The work of Sherman (**26**) and of Pratt, Nelson and Sun (**23**) indicates that the early belief in even three different native emotional patterns in the infant was unduly optimistic. The painstaking studies of Landis (**15**, **16**) on facial expressions and visceral responses should prove beyond reasonable doubt that physiological measures now current do not distinguish the specific emotions.

The studies of Brunswick (**5**), Landis (**15**), and others may be cited to substantiate the statement that smooth muscle tonus, skin conductivity, blood pressure, etc., do not differentiate emotional patterns of response. The mechanisms functioning in strong emotions seem equally important in all responses of that general type. True, certain gross discriminations can be made, as, *e.g.*, between 'rage' and 'love,' but these can also be made by overt behavior and by introspective technique. But specific emotions cannot be so identified.

Introspective evidence is in confusion. Some individuals report qualitative distinctions between emotions other than the cognitive factors involved, while other (and often more sophisticated) investigators report that when the cognitive aspect is excluded, many different emotions 'feel' the same. The very fact that individuals disagree on this point is fairly satisfactory proof that we cannot rely upon introspection alone to differentiate the emotions. On the other hand, it must always remain true that an emotion is definitely a conscious phenomenon, and definitions external to the conscious process must be framed with care.

When we study the correlation of emotions and their expression through the disturbances of psychopathological states, we find even further problems which are unsolved by either the James-Lange or the Cannon-Dana theory. A patient's verbal and overt behavior may not correspond at all. He may laugh boisterously and tell you that he does not feel at

[1] As this work has already been summarized by Dashiell (Are there any native emotions? PSYCHOL. REV., 1928, **35**, 319–327), we need only mention the various lines of evidence bearing on this point.

all merry. One may show no emotion to normally adequate stimuli, or strong emotion to normally subliminal stimulation.

These varying lines of evidence force us to the conclusion that *emotions, as patterns of response, do not exist.* This is a rather obvious conclusion. The force of tradition and an incapacity for looking beyond words are the chief factors which have prevented its earlier acceptance.

Our own theory is that the answer to this problem of emotions involves the resurrection of a long-buried topic: the psychology of feelings.[2] An understanding of this topic is essential to the comprehension of the affective consciousness and emotional behavior.

The only differentiæ of emotional from cognitive processes are the particular feeling-tones which form the experiential matrix. The *specific* emotions can only be differentiated in terms of the perception of the stimulus and its meanings.[3] It follows, then, that a particular emotion can be described as a perceived situation plus a definitive feeling-tone. Allport (1) suggests a theory in which emotions are differentiated into two main groups on the basis of the fundamental feeling-tone as pleasant or unpleasant. He differentiates specific emotions on the basis of proprioceptive sensations.

He assumes that pleasantness is correlated with afferent impulses which follow visceral responses activated by the parasympathetic and unpleasantness with responses dependent upon the sympathetic. It would appear that Allport, attempting to be a good behaviorist, entangles himself needlessly in these peripheral processes and thereby ignores the fundamental role played by the thalamus.

Psychology of Feelings

On the basis of introspective evidence it appears that consciousness is composed of at least two kinds of processes, sensations and feelings. These conscious elements differ from each other in certain fundamental ways: the feelings are less

[2] Cason (8) has recently reopened the topic, but without taking any definite stand.

[3] Newman, Perkins and Wheeler almost convinced themselves of this point, but not quite.

clear than the sensations; they are opposites in the sense that contrasting feelings cannot appear simultaneously in consciousness (Titchener); they last for a greater period of time after the disappearance of the external stimulus; [4] and they are not localizable in space nor referable to some body surface as are sensations.

Further, all (or nearly all) sensations, whether of the somatic or visceral series, appear to have an agreeable or disagreeable quality (feeling-tone). Not only may the sensation in itself be painful or pleasurable, but the related apperceptions, ideas, etc., may have an agreeable or disagreeable concomitant (Herrick). This is especially true of the visceral sensations, which often manifest strong feeling-tone and also that lack of definite localization which characterizes true feelings.

The close connection of sensation and affect prompted McDougall's theory of the relation of emotion to instinct. While the observed data are quite unfavorable to McDougall's hypothesis as stated by him, this close connection must be accounted for in any theory.

The existence of true feelings apart from sensations cannot be questioned. The classification of affective states, however, was a center of psychological controversy for years.

Wundt suggested that there existed three pairs of feelings: pleasantness and unpleasantness; excitement and depression; and tension and relaxation. Titchener admitted only one pair of feelings, pleasantness and unpleasantness; he denied the others both because they were not introspective opposites and because their existence was not indicated by experiments using pneumographic and sphygmomanometric methods. Our own belief is that excitement and depression are as truly introspective opposites as are pleasantness and unpleasantness; and as for the objective experiments, these scarcely favor Titchener's hypothesis more than Wundt's.[5]

We wish, therefore, to suggest that there are four feelings

[4] This point is debatable, but much evidence favors it.

[5] We agree with Titchener in omitting tension-relaxation, but only because we believe that this can be satisfactorily explained on the basis of proprioceptive sensations, which, as will be shown, are peculiarly deficient in feeling-tone.

which can be described and differentiated from each other. Introspectively they may be readily recognized and distinguished, both from each other and from other mental processes; anatomically they differ from sensations as the primary projection areas for sensations lie in the cerebral cortex, while the primary projection areas for feelings lie in the thalamus.[6] Physiologically the feelings are differentiated from sensations in that the reflex responses characteristic of the former tend to be diffuse and generalized, whereas those of the latter tend to be discriminative and differentiated. These four feelings may be given the names from traditional psychology which most adequately represent their introspective nature: pain-unpleasantness,[7] pleasure-pleasantness, excitement and depression.

Anatomical Localization of Feelings

The localization of sensations in the cortical projection areas of the appropriate sensory tracts is a matter of common psychological knowledge, and this point of view is supported by a wealth of material such as the experiment of Cushing on the electrical stimulation of the somesthetic area of conscious patients, the work of Marie and Chatelin and of Gordon Holmes on lesions in the visual area, and the experiments of Dusser de Barenne on stimulation of the somesthetic area of the cat and monkey by strychnine. As the unfortunate result of an apparent process of overlearning, and in memory of an old tradition, many psychologists still believe that all conscious processes are dependent upon cortical functions; yet the critical observations of Dejerine and Roussy (**25**) and of Head (**12**), made over twenty years ago, demonstrated that the seat of the affective consciousness lay in the thalamus.[8]

Lesions in the thalamus, characteristically localized in only

[6] It should be noted that Head has advanced the view that sensations may also have a diffuse representation in the thalamus, with some vague localization.

[7] We suggest the rarely-used term 'algesia' as a more convenient name for pain-unpleasantness, and for those who find difficulty in pronouncing 'pleasure-pleasantness,' the term 'hedonia' is suggested.

[8] Cannon was sure of this in 1927, but seems to have forgotten the fact by 1931. Newman, Perkins and Wheeler say that it is 'dangerous doctrine' to have consciousness in various parts of the nervous system. Since when have 'doctrines' stood in the way of accepting clinically determined facts?

one side, interrupt the sensory paths relayed at this point from the mid-brain to the cortex. The loss of sensation, whose extent and nature depend on the size and location of the lesion, differs in no way from that produced by interference with sensory impulses, either as they enter the optic thalamus or as they pass to the cortex by way of the internal capsule. But to these familiar defects another factor may be added when the lesion destroys certain parts of the optic thalamus. This fresh symptom, which can be attributed only to the disturbed activity of this organ, is a tendency to react excessively to *affective* stimuli. The prick of a pin, painful pressure, extreme heat or cold, produce more distress (on the affected side) than on the normal half of the body. Not only are the responses to pain, both conscious and reflex,[9] exaggerated, but the patient may also respond to pleasing stimuli in this extreme manner. Moderate degrees of thermal stimulation may produce feelings of pleasure on the abnormal half of the body, although no such manifestations are produced when these stimuli are applied to the normal side.

A number of lines of evidence indicate that these exaggerated affective responses actually result from the activation of thalamic nuclei and are not the result of processes of irritation set up in the thalamus and relayed from there to the cortex. The lesions in the thalamus associated with this syndrome are not irritative but stationary; the over-reaction comes on, as a rule, at a considerable time after the onset of the hemiplegia, after the period which might be classified as a shock, and usually lasts unaltered for years. Furthermore, according to Head, stationary cortical lesions which cause no convulsions or other signs of irritation or shock produce no effect on sensitivity to pain. Destruction of the cortex alone does not affect the threshold for the painful or uncomfortable aspects of sensation. Finally we have the negative evidence of Cushing (**9**), who found that direct electrical stimulation of the somesthetic and motor areas of the cortex was not attended by *any* painful sensations, nor by any responses indic-

[9] Obviously only the reflex responses can be shown to occur in mild anesthesia, etc., which situations have bothered Newman, Perkins and Wheeler. The individual was unable to make a verbal (*i.e.*, 'conscious') report.

ative of affective states; as contrasted with Pachon and Delmas-Marsalet (21), who obtained reaction patterns which they described as indicative of a state of satisfaction or pleasure, upon direct stimulation of the thalamus.

Head believes that the reason for the over-reaction in cases of thalamic syndrome is that injury of the lateral nucleus releases this center from the normal inhibitory influences of cortical control. The centers for actual appreciation of the affective process would lie in the medial and anterior nuclei, which (together with the geniculate bodies and the pulvinar nucleus) escape serious involvement in this syndrome.

Pain-Unpleasantness

The first feeling that we shall discuss is that of pain-unpleasantness. The fundamental point to be established here is that pain, usually classified as a 'sensation,' and unpleasantness, usually called a 'feeling,' are not essentially different in kind, but represent only quantitative differences in a single category of experience. Furthermore, we should not distinguish 'psychic pain' from the pain-unpleasantness caused by physical stimuli, but hold instead that the neural processes innately activated by the second class of stimuli are similarly aroused (although, in the usual case, to a less degree) by the former stimuli through a process which may be denominated 'conditioning.'

The same position has been upheld by Ribot (24): "There is a fundamental *identity* between physical and moral pain . . . they only differ from each other in the point of departure, the first being connected with a sensation, the second with some form of representation, an image or an idea." Other psychologists, physiologists and neurologists, assuming a fundamental distinction between the two, have evolved a series of more or less fantastic schemes to account for the apparent similarity.

Titchener (27) writes as follows: "The writer hazards the guess that the peripheral organs of affection are the free afferent nerve endings . . . (which) represent a lower level of organic development than the specialized receptive organs, or

organs of sense. . . . If the hypothesis (that the free nerve endings of the epidermis are to be regarded as terminal organs for pain) is correct, then there is a close genetic kinship between the sensation of pain and unpleasurable affection."

Herrick (13) has attempted to account for the apparent similarity on the basis of nerve paths rather than on kinship between receptors, and he states that "the fact that the primitive pain path in the spinal cord seems to follow a rather diffusedly arranged system of fibres in the fasciculus proprius, frequently interrupted by synapses in the grey matter with correspondingly high resistance to nervous conduction, is perhaps correlated with this general and diffuse quality of unpleasantness. . . . Pain, considered as a distinct sensation, was born out of this situation or differentiated from it. . . . Thus arose pain nerves (if such exist separately) and the pain tract of the spinal cord (whose anatomical distinctness seems well established) and also a special mechanism for painful reactions in the thalamus." It should be noted that this 'primitive' pain path referred to is that of the *cat*, which certainly does not represent a simple type of neural organization; and if Herrick is right, a cat cannot experience pain, but only a modest unpleasantness.

Although both of these investigators have perceived a certain relationship between pain and unpleasantness, they believe them to be fundamentally different; their arguments reduce to the fact that the two are introspectively dissimilar. Unpleasantness is a 'feeling'; it lacks clearness and is poorly localized, if at all; whereas pain is a sensation, clear, localized and (at least according to Titchener) possessing, in the case of cutaneous pain, three qualities: itch, prick and pain proper—qualities which are by no means necessarily unpleasant.

Having separated the feeling of unpleasantness from the sensation of pain, each of these authorities is faced with the task of trying to find a central process whose activation will give the appropriate experiential concomitant. Titchener avoids this issue and leaves the feeling of unpleasantness stranded in the free nerve endings with no apparent hope of rescue.

Herrick meets the problem openly and suggests that, although pain is aroused by a special mechanism in the thalamus, unpleasantness is a result of "a stasis in the nerve centers . . . brought about by a conflict of two sensory impulses for the same final common path, by the dilemma occasioned by the necessity for discrimination in an association center between two or more possible final common paths, by fatigue, auto-intoxication or other physiological states . . . and by a variety of other causes." Such a theory [10] presumes that the neural correlate for a feeling is a motor discharge in the central nervous system and is, in effect, a return to the time-worn (if not time-honored!) 'innervation sense' of Wundt. All the evidence on this point is opposed to such a view; the experiment reported by Cushing on stimulation of the motor area of a conscious patient is especially unfavorable to it. It is difficult to understand why Herrick suggests this hypothesis unless he believes that, since a feeling has no localization in consciousness, it should have no localization in the nervous system!

The fundamental reason why pain is classed as a sensation and artificially differentiated from the feeling of unpleasantness (feeling of pain-unpleasantness, according to our classification) is that the normal (intact) individual is unable to unravel the *feeling* of pain from all the other elements of experience simultaneously aroused by stimulation of a skin surface; and, as a result, what the normal human being reports as pain is in reality a more complex perception in which the feeling of pain is not distinguished from the accompanying tactual sensations (which give it a local sign), from the kinesthetic, thermal and pressure sensations (which give it a definable quality, as grinding, burning, etc.) and finally from the *knowledge* of the stimulus (associations initiated by stimulation of both contact and distance receptors) which imposes still further limitations upon the original crude *feeling* of pain. So complete is this fusion that, as in the case of taste and smell, introspection is normally inadequate to separate the various elements.

[10] Markedly similar views on the introspective side have been upheld by Paulhan (22), MacCurdy (17), Cason (8) and many others.

Fortunately we have a condition in the syndrome thalamique in which these two fundamental processes are partially or completely dissociated, and the patient is enabled to give introspective evidence on the nature of the consciousness of pain independent of the masking effect of associated cutaneous or kinesthetic sensations.

Under these conditions pain loses all the characteristics of a sensation, takes on the attributes of a feeling, and can be differentiated from unpleasantness only on the basis of intensity. The capacity to localize pain is closely related to the integrity of the associated somesthetic pathways. In those cases of syndrome thalamique in which these pathways are relatively uninjured, the locus of the 'sensation' of pain is about as exact as in the normal person, but if the pathways in the fillet are seriously involved, the localization becomes extremely imperfect, the painful 'sensation' radiates widely from the spot stimulated and may seem to involve an entire appendage or the entire afflicted half of the body. Under such conditions it must certainly be classed as a feeling.

The phenomenon of referred pain seems also to support the hypothesis that the localization of pain is dependent upon the associated cutaneous stimulation. Szemzo (see **14**), on the basis of clinical observations, concludes that the mechanism involved here is a hyper-irritability of the spino-thalamic system.

In exactly the same manner, the *quality* of the 'sensation of pain' disappears or is falsified if the associated kinesthetic or cutaneous pathways are involved, and if information through exteroceptive stimulation is cut off. One patient describes mild tickling on the sole of the foot as follows: "It's burning; it's as if you were tearing the skin off; it sends pins and needles up the leg as far as the knee." Another describes a prick as "something burning" while ice produces a pain "as if something pricked me and made me jump." When the left sole of the same patient was scraped with the finger nail, he showed signs of distress and said, "*I don't know what you are doing, but it affects me all up the side.*" Another patient, who showed extreme loss of cutaneous sensitivity, responded

to stimulation by tubes containing water below 20° or above 55° C. by screwing up his face as if in extreme pain and crying out, "Oh, something has caught me" or "Something is forcing its way into me" or "It's got hold of me. It's pinching me."

It should be noted in these cases that stimuli that a normal person would report as *unpleasant* are reported by these patients as being very decidedly *painful*, giving definite evidence that these are not two separate categories of experience (as they are induced by the same stimuli and by activation of the same cerebral areas) but that they are only quantitatively different: a single feeling differing in the attribute of intensity. Further, we do not have to rely on a comparison of one individual with another, for we have within the same patient normal and affected areas as a perfect experimental control. Prick is invariably *more* painful on the affected side of the body; tickling, which may be indifferent or unpleasant on the normal side, is distinctly unpleasant or painful on the abnormal half of the body; noises previously indifferent now become distinctly disagreeable, and in one case the noise of the railway on which the patient was traveling became so intolerable that he attempted to throw himself out of the train.

Furthermore, it should be noted that mental (psychic, 'moral,' associated) pain, which has only the quality of unpleasantness to the normal person, frequently engenders in the syndrome thalamique an experience of 'physical' pain and not unpleasantness. Head describes the behavior of one of his patients in the following words: "She objects intensely to anyone sitting to her right side, for she then begins to suffer from the same disagreeable sensations in this half of the body that were evoked by contact. When one of us placed himself to her right without touching her, she complained of a 'soreness all down the side, *as if you were pulling a dressing from a wound.*' This ceased entirely when the observer moved to her left. This phenomenon seems to be not uncommon in this class of cases and we have met with it in two other instances."

Emotional music may produce exaggerated affective states when no other auditory stimulus produces an abnormal affect. In summary, and as Head himself states, "Not only does the

abnormal half of the body respond more vigorously to the affective element of a stimulus, but an over-reaction can also be evoked by purely mental states."

Pleasure-Pleasantness

The general confusion of unpleasantness with 'pain' is not paralleled in the case of pleasure-pleasantness, probably because the stimuli normally adequate to arouse this feeling do not uniformly arouse any particular set of cutaneous or other sensations. The feeling of pleasantness is diffuse and unlocalized and, as Titchener says, "coextensive with consciousness. . . . If the pleasantness of a taste is localized in the mouth, that is simply because consciousness itself has been narrowed down to a taste-consciousness."

Extreme psychological theorists have, however, attempted to reduce pleasantness to sensations. Stumpf, for example, offered an explanation of pleasant feelings in terms of tickle and a weak sensation of lust. Such a theory obviously does not account for the pleasant feeling tone of certain olfactory and gustatory stimuli, musical tones, processes accompanying digestion and elimination; and it ignores the fact that the fundamental feeling-tone of all these situations is the same.

Nafe (**18**) has also attempted to analyze the feelings of pleasure and unpleasantness into sensations, and on the basis of presumably careful introspective studies concluded that "under the conditions of the present experiment" (the feeling of) "a pleasant experience is bright pressure or a touch quality lying between bright pressure and tickle; in the case of an unpleasant experience, the increment (feeling) is a dull pressure or quality lying near dull pressure."

The same author again suggests that "at all intensities it is known that pain is characteristically unpleasant. It seems possible, if not probable, that the unpleasantness itself consists of the experiences aroused by typical patterns of reflexly aroused responses."

With so few somesthetic modalities to choose from, it seems a shame that Nafe (or his subjects) should identify unpleasantness with proprioceptive sensations, which Head has shown to

be entirely *without feeling-tone*, and pressure, which is without feeling-tone unless *accompanied* by 'deep pain.' Likewise it seems unfortunate that Nafe should identify pleasantness with pressure and with tickle, which appears never to be a stimulus for pleasant feelings, and almost invariably, in the case of Head's patients, acts as a stimulus for *unpleasant* feelings.

Bousfield (**4**) suggests that pleasure is the result of a discharge or release of tension, that it is negative in character and consists simply in the release from pain. The same view is supported in the recent article by Cason (**8**), although the latter makes the reservation that some pleasures, as, for example, in tastes, odors and tonal intervals, have a positive character in themselves and are definitely raised above the so-called zero- or indifference-point in feeling.

Plainly neither Bousfield nor Cason have considered the neurological implications of this theory. If pleasure is a release from 'visceral tension' and nothing more, then the consciousness of pleasure must be the concomitant of the discharge in these encephalic areas, and as we have previously pointed out, the available evidence is decidedly against such a possibility.

Our own point of view is that mere release from tension or pain can only result in a neutral feeling-tone, since this would result in discontinuance of thalamic activity; and that the feeling-tone of pleasure often ascribed to these situations invariably results from positive activities associated with or following the release phenomenon.

Bousfield's favorite example, pleasure following the 'release from tension and accompanying unpleasantness' of the hunger contractions, fails to support his own hypothesis and does support ours. If the incidence of hunger contractions is not followed by ingestion, they disappear of their own accord and are *not* accompanied by any positively pleasant feeling, or the hunger pangs may be stopped by swallowing certain indigestible materials, *e.g.*, bitter alkaloids, which produce a feeling of unpleasantness. The violent unpleasant emotions also bring about inhibition of stomach contractions which is

not accompanied by pleasure. Indeed, it is entirely unconscious—a typical negative state.

The positively pleasant aspect of ingestion is, instead, to be attributed to the pleasant feeling-tone associated with olfactory and gustatory qualities of the food materials and with pleasantness from associated activities of digestion. These last might be ascribed to visceral sensations of warmth induced by increased distribution of blood in the splanchnic area.

In the cases of syndrome thalamique reported by Head, over-reactions to pleasant stimuli were not uncommon and were given in response to tactile, auditory and thermal stimulation. In the case of the thermal stimuli the pleasant feeling-tone might be aroused while the patient was unaware of the nature of the stimulating object or even that he was being subjected to thermal stimuli, and the experience was described in such phrases as, "Oh, that's lovely, it's so soothing" or "Oh, that's exquisite" or "That's real pleasant."

The existence of pleasurable experiences, independent of other forms of conscious responses to the stimulating object, and possessing the characteristic attributes of a feeling, proves that this feeling is a true element of experience and is not to be confused in any way with either simple sensations or the 'release of tension.'

Purely mental states can arouse typical feelings of pleasure in the syndrome thalamique in the same manner as they arouse feelings of pain-unpleasantness. Such is the case of a highly educated patient who confessed that he had become more amorous since the attack, which had rendered the right half of his body more responsive to pleasant stimuli. "I crave to place my hand on the soft skin of a woman. It's my right hand that wants the consolation. I seem to crave for sympathy on my right side. My right hand seems to be more artistic."

In view of this evidence, we think the conclusion logical that pleasure-pleasantness is a conscious feeling concomitant of some process in the thalamus.

Excitement

The thalamus appears to be the center not only for the conscious and reflex expression of the feelings of pain and pleasure, but also for those of the antagonistic feelings of excitement and depression.

The existence of an 'excitement' center in the thalamus finds objective support in the experiments of Bard (**2**), who obtained a reaction of strong 'emotional' excitement in the cat (46 successful observations) following ablation of the hemispheres, corpora striata and the cranial half of the diencephalon. Four animals showed the 'sham rage' following ablation of the dorsal half of the diencephalon, although the skeletal muscle components of the responses were somewhat limited, and spontaneous excitement did not appear as readily as in the less severe operations. This evidence makes it appear that only the ventral and most caudal fractions of the diencephalon are essential to bring about the remarkable group of responses which Bard calls 'sham rage,' since total ablation of the cerebrum abolishes the response.

Although Bard calls the state of emotional excitement 'sham rage,' the essential characteristic of his preparation is a state of increased bodily activity dominated by violent sympathetic discharge and it is fully as apt to characterize the behavior of this animal as representing excitement (feeling of excitement, if conscious) as to ascribe to the animal a particularized emotion of rage. Removal of cortical control has apparently left the animal in a state of chronic excitement (which we feel is more aptly described as a feeling than as an emotion, since the latter is characteristically induced by some external animal or object) which seems to predispose the animal to react in an emotional manner to a wide variety of stimuli, many of which, as in the spontaneous fits of rage, are well below the threshold for the normal cat.

Bard describes the behavior of his cats as follows: "The pseudaffective phenomena . . . usually appeared in fits which lasted from a few seconds to several minutes. They included struggling, attended by movements of the head and arching of the trunk, with thrusting and pulling of the limbs; clawing

movements of the forelegs with protrusion of the claws, waving and lashing of the tail; a snarling expression; and very rapid panting with mouth open and movements of the tongue to and fro. In addition to these activities were signs denoting a vigorous sympathetic discharge: erection of the tail hairs; sweating from the toe pads; retraction of the nictitating membranes; exophthalmos (separation of the lids); large increments in arterial pressure and heart rate, and pupillo-dilatation in some cases."

The greater number of these responses would characterize the emotion of 'fear' as adequately as they would the emotion of 'rage,' and in view of the work of Landis (**16**) in differentiating emotional responses in the human being, it is more than doubtful if naïve observers would more frequently identify the behavior of these cats as exhibiting rage than fear. Bard's cats, it should be remembered, were also subjected to a constant 'rage' stimulus, as the four limbs were bound to an animal board. Were this stimulating condition removed, even greater difficulty should be encountered in determining the 'emotion' experienced by the cats. It is unlikely, however, that anyone could cavil at the application of the term 'excitement' to this condition, and its nature fits so perfectly the rules laid down for the definition of a feeling that we consider it an excellent example.

Both Bard and Cannon consistently refer to these conditions as emotional (the thalamus as the 'seat of the emotions,' etc.) and Cannon especially misuses the term 'emotion' in referring to feeling states. This variety of psychological naïveté is found only too frequently in physiologists doing psychological experimentation, although it is no doubt equalled by the malapropisms of others. Bard, indeed, emphasizes the generalized nature of the phenomenon and stresses the absence of discriminative reactions; in his summary, he goes only so far as to state that the decorticate behavior 'resembles' rage, which is a very wise reservation.

The most important cortico-thalamic tracts in man lead from the prefrontal lobe to the thalamus, and we might therefore expect that frontal injuries in man would result particu-

larly in affective derangements. Feuchtwanger (10) made observations on 200 frontal and 200 non-frontal injuries and his results tend to support the view that the cortico-thalamic connections are essential to a normal affective condition. The most frequent differential symptoms of frontal lobe injuries include excitement and euphoria, depression, slowness, and apathy.

An excellent example of the production of states of excitement in a human being following involvement of the basal ganglia is that reported by Fulton and Bailey (11) as occurring in a boy following removal of a tumor in the third ventricle. The patient was "very restless, rolling and tossing constantly; crying and whining incessantly"; this condition persisted for two weeks. In other cases in which great psychomotor excitation resembling more or less closely the classical mania of psychiatry has been described, the tumor has usually been situated along the brain stem, *e.g.*, in the hypophysis (Boyce and Beadles), the third ventricle (Rychlinsky) and corpus striatum (Parker, Clouston, Furstner, Oppenheim, Randot), the thalamus (Pfeifer), the pineal body (Klebs, Goldzieher, Neiden), the pons (Schuster, Dreshfeld, Boyer) or the cerebellum (Hermonides, Steiner).[11] The authors also report three cases of maniacal excitation after operations for hypophyseal adenoma.

Depression

Excitement appears in these cases more frequently than depression, possibly as a result of the constant irritative effect of the disorders. In the chronic physiological preparation (Bard, Schaltenburg) no evidence can be obtained for the existence of depressed states.

Pathological evidence clearly indicates, however, that involvement of these hypothalamic areas may induce feelings of depression and related states. Thus in case IV of Fulton and Bailey, the complete emotional apathy induced by widespread involvement of the third ventricle was preceded by a "rather moody and somewhat distant" disposition on the part of the patient. Marchand and Chatagnon (see 6) record a case in

[11] The above cases are reviewed in the article by Fulton and Bailey (11).

which involvement of the hypothalamic area was first attended by strange phobias, anxiety and depression, and Roussy and Lhermette (see 11) record a remarkable case of a young girl, suffering from involvement of the tuberian region, who became somber and taciturn, with ideas of suicide. She had previously been gay and active. In the midst of her depression she would have spells of exuberant gaiety, terminating in motor agitation, constriction of the pharynx and a flood of tears. Her whole affectivity was troubled, not only her sentiments toward her parents, but also her religious feelings, which passed from an exalted devotion to an absolute indifference.

The previously mentioned study of Feuchtwanger indicates that in human beings release of these centers from cortical control results almost as frequently in emotional depression as in emotional excitement. The apparent similarity in anatomical location and the rapid transitions frequently seen in these states of feeling make it seem probable that excitement and depression are closely related.

In paresis affective changes are brought out with exaggerated emphasis. The particular reaction type of the individual case may be euphoric, excited or depressed, but we have been unable to obtain carefully reported autopsies with diagnosis. The lesions are characteristically in the frontal and prefrontal lobes and degenerative changes (sanding) are found in the ventricles, particularly in the third and fourth. In the other classical pictures of insanity no lesions have been demonstrated. Courtney prefers to explain this in terms of pathological changes in the encephalon which have not yet been demonstrated, but such a view is not at all essential to the affective theory here propounded. Affective responses may be 'conditioned' and these conditioned feelings obey the usual laws of learning in terms of which recent psychologists (*e.g.*, Morgan) have attempted to explain the functional psychoses.

Implications [12]

We have so far failed to express explicitly the implications of this theory for the psychology of emotions. Certain points,

[12] The theoretical attitude toward emotions which follows logically from the theory of feelings here presented will be elaborated in a further paper.

however, may be mentioned. In the first place, the similarity of the violent emotions is explained in terms of the excitement component. The pathological conditions of disturbed correlation between feeling and expression are due to injuries in the diencephalon and its accessory structures. The introspective differentiation of emotions is seen to depend on the perceived situation plus the feeling-tone contributed by the arousal of thalamic processes. The profound visceral concomitant of most emotions is seen to be due to the intimate connection of the diencephalon with the sympathetic nervous system. The physiological studies of the thalamus fit better into a descriptive scheme of feelings than emotions, since the behavior manifested does not correspond to that which may be identified as emotional.

Summary

1. We have attempted to show that there are four differentiable feelings: pain-unpleasantness, pleasure-pleasantness, excitement and depression.

2. The anatomical seat of these processes lies in projection areas of the thalamus, the first two lying probably in more anterior regions than the latter.

3. Pain, either physical or mental, differs only in intensity from unpleasantness.

4. Pleasure and pleasantness are similarly identifiable.

5. The emotional consciousness has a fundamental tone imparted by these affective states.

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DISCUSSION

WHAT IS A DISTRACTOR?

I

Distractors became of great psychological interest in connection with experiments on attention. The distraction method was proposed as a technique for measuring the degree of attention (4). This method never succeeded because it was impossible to obtain a satisfactory distractor. Distractors next became of interest in reaction-time experiments. If, under constant conditions, an experimenter found that certain reaction times were greatly lengthened, distracting stimuli was the explanation. Many investigations were undertaken to determine the effect of distractors on reaction-time. Divergent results were obtained. One investigator found that distractors lengthened reaction-time, another that they had no effect, and yet a third found that distractors shortened the reaction-time. Cassel and Dallenbach (1) state: "The effect upon reaction-time of intercurrent or 'distracting' stimuli has been variously reported. Most frequently the 'distraction' serves to lengthen the time; but sometimes it decreases it; and occasionally, after a brief initial disturbance, it leaves the time unaffected."

The concept of distraction was immediately adopted by workers in other fields and accepted uncritically. Witness the naïveté of the following statement: "To determine distraction effects it would therefore only be necessary to administer a reliable intelligence test under distraction conditions" (2). Other writers, attempting to evade the term, *distractor*, indulged in circumlocutions. "Our endeavor will be to discover the way in which a subject will react to irrelevant noises which are produced while he is engaged at some task" (3). Here, 'irrelevant noises' are substituted for distractors; but 'irrelevant noises' also cry for definition.

II

Before proceeding it will be well to clear the ground of the objectionable term, distractor. When psychologists call something a distractor it is assumed that the object inherently has the power to distract one's attention. This assumption is known to be false.

Certain noises which are disturbing to some may have no effect whatever on others. People accustomed to working under noisy conditions will not find noise a distractor; on the contrary, quiet would be disturbing. Again, certain noises may be distracting at their inception and then lose their potency for distracting the attention; they cease to be distractors. Further, all distractors are intermittent distractors, for they cannot divert the subject's attention during the entire course of the experiment; if they did the subject would not be able to perform the task set before him. It is now clear why a distractor cannot be uniform in its effect. It is evident, then, that in the present state of discourse we cannot speak of distractors or even distracting stimuli. We are only justified in speaking of the distracting effects of stimuli. The question is now: "Distracting effects on what?"

Historically, experimenters busied themselves with distracting effects of stimuli upon attention. After that they became interested in distracting effects on performance, but usually assumed that there was an intermediary factor of attention. The stimuli distracted attention, and distracted attention caused lowered performance. Assuming a fixed and constant relation between attention and performance, lowered efficiency was proposed as an objective measure of attention. Later, experimenters spoke of distracting effects on performance, omitting all reference to attention even though they still called the stimuli distractors.

As I see it, two alternatives are open to psychologists. Either they accept the common usage of the term, distractor (in which the effects of the stimulus refer to attention), or they may speak of the distracting effects of a stimulus upon performance. The former alternative will now be considered.

If psychologists speak of distracting effects of stimuli upon attention they will remain in agreement with psychological thought of the early years of this century and with the common usage of the term distractor. The paradoxical results obtained by experiments on distractions will then be readily explicable when it is borne in mind that the effects of the so-called distractors vary from individual to individual and within one individual at different times.

The second alternative, defining the word *distractor*, so as to refer to performance, I consider the better. In the first place, it is in harmony with the present-day use of the term in psychology, and in the second place it avoids all reference to attention. The relationship between attention and the stimulus is unknown and the rela-

tionship between attention and the response is also unknown. Therefore, it greatly simplifies matters to omit reference to an immeasurable, hypothetical entity.

If we accept the second alternative, the word *distractor* will have a non-ambiguous meaning. A distractor is a stimulus which exerts a deleterious effect on performance or efficiency. If ringing a buzzer causes lowered efficiency, the ringing buzzer may properly be called a distractor under those particular conditions. If ringing a buzzer has no effect on the performance, then the buzzer cannot be called a distractor. When it is remembered that a distractor is a stimulus which exerts a detrimental influence on efficiency, a statement that distractors sometimes aid efficiency and sometimes have no effect on efficiency will be meaningless. If efficiency has not been lowered the stimulus is not a distractor.

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THE ROLE OF MASS ACTIVITY IN THE DEVELOPMENT OF INFANT BEHAVIOR

We wish to offer some criticisms of a view of infant behavior which has been presented recently by O. C. Irwin (4, 5, 6). In brief this theory is that 'mass activity' is the primary infant activity and that through a process of 'differentiation' it is transformed into the more restricted responses. Essentially this is an attempt to apply Coghill's concepts to infant behavior. A fuller presentation of Irwin's notions of 'mass activity' and of 'differentiation' can best be given in his own words. He describes mass activity as follows (5, p. 198): "The trunk is thrown about, curved, flexed, extended, arched, and rolled from side to side. The movements of arms and legs merge with those of the trunk with great variability, being slashed and kicked, flexed and extended, adducted and abducted incessantly. Ankle, wrist, elbows, knees, fingers, and toes as well as facial muscles are involved as part of the larger movements. Crying usually accompanies them also. There are no regularities, no fine and precise gradations, little specificity in the reflex sense, and few effective coordinations in this activity. To designate the type of reaction involving the whole organism, whether proceeding at a high or low rate, I have used the expression 'mass activity.' It stands in striking contrast to the movements of single segments of the infant. The latter type of reaction is called specific." To us a better name than mass activity would seem to be the usual one of restlessness and crying, but the more high-sounding designation is gaining wide acceptance.

Concerning this activity, Irwin writes (5, p. 201): "Differentiation and specificity of behavior in infants, therefore, probably develops out of an original matrix of mass activity. Mass activity probably is the earliest activity of the uterine organism. Segmental movements are aspects of it which become partly specialized in the uterine environment and are evident in the newborn. Nevertheless, at birth there is a great deal of nonspecific behavior."

We would make clear from the beginning that we agree fully with Irwin's view that the earliest human responses are total bodily responses as opposed to isolated reflexes. In fact, Coghill himself in an article (1) not cited by Irwin has applied his views to human

fetal behavior. Our objections are wholly concerned with the importance of restlessness and crying. They may be divided into three parts.

In the first place one may judge from Irwin's neglect of other responses that he thinks of mass activity as the only total bodily response exhibited by newborn infants. If this is his belief, we are convinced that he is mistaken. In a summary of the literature (3) we found that at least fifteen responses involving a characteristic participation of the trunk and limbs have already been described. These descriptions are being verified by research in this laboratory. Further studies of our own show that nursing, defecating, gagging, and being handled are each accompanied by a characteristic total bodily response in the newborn. Nor does this complete the list. While our data cannot be presented here, we believe they will be conclusive.[6]

If the newborn infant presents such an array of total responses, why should mass activity or any one response be selected arbitrarily as the sole source of more localized behavior? In Irwin's case we believe the answer lies in a certain misunderstanding of Coghill. This misunderstanding is that all behavior results from the expansion and individuation of a *single* total response. In fact Coghill's usual statement (2, p. 38) *is* that behavior develops from a perfectly integrated total pattern (through expansion and individuation), but we believe this to be merely a generalized statement of process with no implication that there is only one response from which behavior develops. The opposite interpretation is encouraged by the fact that Coghill's work deals almost exclusively with locomotion in Amblystoma; but he does, however, deal with the feeding response as a separate reaction, and in the case of human fetuses he has no hesitation in discussing several responses (1).

Secondly, it may be that Irwin by his neglect of other responses does not intend to imply that mass activity is the only total bodily response of newborn infants. In that case the only claim which he presents for the primary importance of mass activity seems to be the claim that it is the earliest fetal response. In this connection, we first raise a reasonable doubt as to whether the described responses of fetuses may be considered to be mass activity. Another objection arises over the apparent identification of the responses of delivered fetuses with those of fetuses in utero. It is obvious that reactions of extracted fetuses undergoing asphyxiation may be very unlike those of fetuses in position. Of the behavior of the latter, we know noth-

ing. But considering the fact that hunger and noxious stimuli seem to be the chief stimuli to mass activity in the newborn (Irwin, 4) it seems extremely unlikely that mass activity is a prominent fetal response.

Thirdly, Irwin's conception has the tremendous difficulty of not showing how reflexes can be individuated from a total response in which they appear never to be contained, a process not supported by Coghill. Smiling seems never to be a part of restlessness and crying. How then can it develop from that origin? Any number of other responses present the same problem. We scarcely need dwell upon it further.

In summary, we repeat that mass activity has not been proved to be the only total response of newborn infants, that it has not been shown to the first fetal response, and that many responses are not parts of that activity. Therefore we see no reason for assigning it a predominant place in infant development.

We may add that when an infant is permitted to become hungry, as in Irwin's experiment (4), mass activity may be a striking feature of behavior merely because of its frequency, but any response may be made quantitatively impressive by constantly stimulating it. The constant presence of a 'pacifier' in the infant's mouth or of snuff in the air would bring other responses to the fore.

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